DOCUMENT RESUME

ED 263 129 TM 850 594

| | |
|---|---|
| AUTHOR | Carroll, John B.; And Others |
| TITLE | Improving Education: Perspectives on Educational Research. |
| INSTITUTION | National Academy of Education, Pittsburgh, PA. |
| SPONS AGENCY | Ford Foundation, New York, N.Y. |
| PUB DATE | 84 |
| NOTE | 49p. |
| AVAILABLE FROM | The National Academy of Education, LRDC Building, University of Pittsburgh, Pittsburgh, PA 15260. |
| PUB TYPE | Viewpoints (120) -- Collected Works - General (020) |
| EDRS PRICE | MF01/PC02 Plus Postage. |
| DESCRIPTORS | Attitude Change; Educational Change; Educational History; *Educational Improvement; *Educational Policy; Educational Practices; Educational Psychology; Educational Quality; *Educational Research; Elementary Secondary Education; Higher Education; *Instructional Improvement; Policy Formation; Research Design; *Research Problems; *Research Utilization; Social Science Research; State of the Art Reviews; Teacher Education |

ABSTRACT

Seven papers on educational research and educational improvement are presented. Robert Glaser, in the introduction, states that efforts to realize our aspirations for educational change, as well as in the social surroundings and public attitudes that aid school reforms, can be greatly assisted through research. John B. Carroll discusses significant educational research and the process of educational research's impact on education. James S. Coleman offers recommendations for planning educational policy research and for publishing the results. Harold Howe II reflects on the difficulties involved in the acceptance of educational research and the difficulty of convincing political authorities to support it, and presents the concept of disciplined inquiry. Robert A. LeVine describes research in comparative education and human development that has influenced attitudes toward the effects of education. Jacob Mincer considers the role of education in economic growth and its effects on wage structure in the labor market, and discusses the concepts of overeducation and undereducation. Lauren B. Resnick emphasizes the importance of cognitive science in improving instruction. Finally, Lee S. Shulman links research on teaching and learning with improvement of teaching and teacher training. (GDC)

**********
* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *
**********

# Improving Education: Perspectives on Educational Research

John B. Carroll
James S. Coleman
Robert Glaser
Harold Howe II
Robert A. LeVine
Jacob Mincer
Lauren B. Resnick
Lee S. Shulman

U.S. DEPARTMENT OF EDUCATION
NATIONAL INSTITUTE OF EDUCATION
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

☑ This document has been reproduced as received from the person or organization originating it.

☐ Minor changes have been made to improve reproduction quality.

• Points of view or opinions stated in this document do not necessarily represent official NIE position or policy.

"PERMISSION TO REPRODUCE THIS MATERIAL HAS BEEN GRANTED BY

Bennett, B.

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)."

The National Academy of Education produced this report under a Ford Foundation grant supporting activities to promote equality and excellence in education. The opinions expressed are those of the individual authors.

Single copies are available from:

The National Academy of Education
LRDC Building
University of Pittsburgh
Pittsburgh, Pennsylvania 15260

Please inquire about the postage and handling charges on larger orders.

Copyright© 1984 by The National Academy of Education.

# Table of Contents

The Necessity for Research in Education: An Introduction 4

Robert Glaser
University of Pittsburgh

Educational Research: Reflections on the Past 9

John B. Carroll
University of North Carolina at Chapel Hill

How Might Policy Research in Education Be Better Carried Out? 15

James S. Coleman
University of Chicago

"The Value of Research and Disciplined Inquiry to the Improvement of Education": Impressions of a Nonresearcher 21

Harold Howe II
Harvard University

The Value of Comparative and Developmental Research on Education 26

Robert A. LeVine
Harvard University

Overeducation or Undereducation? 30

Jacob Mincer
Columbia University

Cognitive Science as Educational Research: Why We Need It Now 36

Lauren B. Resnick
University of Pittsburgh

Understanding Pedagogy: Research for the Improvement of Teaching and Teacher Education 42

Lee S. Shulman
Stanford University

The Membership of the National Academy of Education 47

# The Necessity for Research in Education: An Introduction

Robert Glaser

*Robert Glaser, the Academy's President, is University Professor of Psychology and Education and Director of the Learning Research and Development Center (LRDC) at the University of Pittsburgh. Professor Glaser is a consultant to national and international commissions and to foundations, and now serves on the Board of Trustees of the Human Resources Research Organization, the Executive Committee of the International Association of Applied Psychology, the National Research Council Committee on Research in Mathematics, Science, and Technology Education, and as chairman of the American Educational Research Association Publications Committee. Recently, Professor Glaser has been awarded honorary doctorates from the University of Leuven, Belgium, and Indiana University, and received the Edward Lee Thorndike Medal from the American Psychological Association Division of Educational Psychology.*

Calls for reform in education resound in our ears. Those of us who would respond look for the information necessary to inform our actions. We are urged toward excellence but observe that, despite inspiring instances of professional excellence, the educational profession is seriously undernourished by modern advances in knowledge. For all its pervasiveness in our society and for the value we place on it, education is one of the least research-supported professions. In other valued services, we would be appalled if practice proceeded without a reasonable level of sustained research, disciplined inquiry, and development by outstanding well-trained scientists, scholars, and technicians. We delude ourselves that educational reform will be sustained and that the teaching profession will be enriched without a research base like that required in other professions. If the current fervor for change is to precede a significant period of improvement, reliance on work in the relevant sciences and disciplines will be essential.

**If the current fervor for change is to precede a significant period of improvement, reliance on work in the relevant sciences and disciplines will be essential.**

Fortunately, some awareness of the necessity for research that supports the teaching profession is apparent among policymakers in the recent reports that have publicized the need for reform. The signal report, "A Nation at Risk" (1983), has suggested the value of a research base. The National Commission on Excellence in Education has given authority to its claims in appendices cataloging studies and statements by researchers. The report of the Twentieth Century Fund Task Force (1983) devoted several pages to "educational research," the final topic of the six taken up in its agenda for federal involvement in education. The necessity for research has been emphasized in the National Science Board Commission's assertion that recent work in the social and behavioral sciences must be considered in setting new educational objectives. "Research into the processes of teaching and learning should be supported with federal funds at both the basic level and at the level of classroom application. This research should further the recent progress in the cognitive sciences, and particular research projects should investigate the integration of educational technologies into the processes of teaching and learning." Its treatments of teaching, teaching materials, and educational technology each refer to the use of research information. This report underscores the necessity for keeping pace with advances in understanding the nature of human intelligence and learning, in studies of the sociology and politics of school management, and in methods for designing and evaluating instructional materials and practices.

A recent meeting of the National Academy of Education considered the challenge of improving education by applying research to practice. At one of its sessions, members from various disciplines presented statements for discussion and their papers comprise this report. The writers are distinguished scholars who have applied their expertise to a variety of educational issues including classroom learning, policy analysis, comparative education, the economics of education, the cognitive psychology of subject-matter learning, the analysis of teaching, and research administration and management.

Their comments give us seasoned perspectives on the ways research and practice mutually engage one another.

**A linear progression from research and discovery, to development, to implementable reform only partially captures science's and practice's influences on one another.**

The clear message of these discussions is that research and disciplined inquiry in fields relevant to education influence educational practice and policy in a manner resembling the influence of knowledge in other fields. In medical practice, for example, research-based innovations in the maintenance of health are subject to change and correction with trial and evaluation, and are subject as well to group pressure and the fashion and fads of the time. A linear progression from research and discovery, to development, to implementable reform only partially captures science's and practice's influences on one another. In education, theories of human development, of the limitations and powers of human memory, and of human thinking and problem solving, for example, represent fundamental principles of cognition that are foundation stones for practice and technology. But these theories, to use the insightful Ernest Hilgard's (1964) analogy, do not dictate instructional practices any more than the principles of thermodynamics dictate whether airplanes shall be driven by propellors or jets. The experiences and problems of practical application inform research as well, and suggest investigations for the development of new theory. Knowledge, technology, and the practitioner's art intertwine in the shaping of science to human purposes. The web of relationships between invention, scientific understanding, and practical application becomes the basis for the knowledge that undergirds the teaching profession.

The papers in this report lead me to some general observations about research in education. First, rapid advances in the social, behavioral, and cognitive sciences have yielded new understanding of human mental functioning. Work on human development, information processing, and cognitive processes such as memory, attention, and problem solving offer powerful reconceptualizations of the nature of the learning process and of the acquisition of subject-matter knowledge and intellectual skills. This knowledge is being applied to the analysis of instruction in the traditional subject matters of schooling – reading, writing, mathematics, and science – as well as to the skills of learning and thinking that should accompany them.

Second, new methodologies are available for the study of teaching and pedagogical processes in schools. The relationship between teacher behavior and student performance, and teaching in relation to instructional materials and computer technology are being studied with new precision. This research activity will undoubtedly change the content and complexity of the principles and practices that comprise the basic education of teachers, and inspire the development of new tools and materials for the profession. A profession of education, supported by scientific and technological research, will require intensive training that will heighten the standards for entrance and internship in teaching as well as qualifications for practicing.

Third, in support of the increasing recognition of the significance of research reflected in recent national reports, it is becoming better and better established that the findings of educational research generate principles and precepts for educational innovation and professional practice. The changes stimulated can be widely influential and result in effects that endure – determining forms and outcomes of schooling that shape habits of living and of thinking. Where outstanding talent drives this research and its practical application, the possibilities for real progress are maximized. However, too often, in the quest for improvement (and the immediate rewards of publicity and recognition), schools introduce and assimilate findings from research that address their problems, without carefully ascertaining limitations and range of application.

Fourth, educational policy is shaped and buffeted by a great many interests and is often influenced by incomplete and irrelevant information. Effective policy, therefore, requires the soundest data that research can provide – research which employs modern methodologies and closely attends to the interests, values, and problems of the parties involved. Policy research should be designed to be immediately and directly useful, unlike long-term research, which is primarily designed for the discovery of new knowledge.

**A significant portion of funding for research in education should be allocated for scientific advancement as it is in other fields. Too many of those concerned with educational reform . . . conceive of educational research funding . . . primarily as a set of activities which should be directed toward the alleviation of immediate or politically significant problems.**

Fifth, over the long term, the research necessary for the improvement of education is like that we recognize as most useful for the normal course of science – generating knowledge that provides the understanding that can lead to invention and innovation. A significant portion of funding for research in education should be allocated for scientific advancement as it is in other fields. Too many of those concerned with educational reform fail to conceive of educational research funding this way; they view educational research primarily as a set of activities which should be directed toward the alleviation of immediate or politically significant problems. Fundamental research, deemed essential for other major professions, comes less readily to mind for education. This attitude is no longer defensible in light of the advances in the sciences and disciplines relevant to the educational process.

In reviewing briefly the essays that comprise this report, we see these general themes reflected. In the first paper, John Carroll points to pervasive older influences on educational practices that have been woven into the foundations of educational theory and sustained or amended as new information was uncovered. He then considers the impact of research of more recent vintage, over the past twenty years, carried out by members of the Academy. Research on school integration, models of school learning and the concept of mastery learning, work on the psychology of reading and analysis of practices for teaching reading, and work on instruction leading to microcomputer technology all testify to the value of knowledge about educational processes. Carroll also describes what appears to be a characteristic course of events in educational research's impact on education.

James Colemen emphasizes the utility of social research for the formulation of educational policy. He distinguishes between research undertaken to advance knowledge, which may involve abstract theory, and the somewhat different forms of research relevant to shaping policy. He outlines means of assuring that social science analyses of education are relevant to the communities served by the schools. Coleman's recommendations for planning policy research and for publishing its results merit close consideration.

Harold Howe reflects on the difficulties involved in the acceptance of educational research and the difficulty of convincing political authorities to support it. He, like Coleman, links the lack of acceptance to a misunderstanding of the distinction between long-term research necessary for the discovery of new knowledge, and research that he refers to as "disciplined inquiry." The latter may not have all the characteristics and rigor of "true" scientific research, but is carefully directed toward understanding a particular situation with which school people grapple. In addition to asserting the value of targeted inquiry on school problems, he emphasizes that the best research in education shares the attributes of the best research in science.

Robert LeVine describes research in comparative education and human development that has informed and altered our views of the effects of education. He mentions anthropological, sociological, historical, and cross-cultural studies, which have broadened conceptions of education from formal tuition in schools to social settings as learning environments. His essay underscores the need for the enlarged vision that appreciation of other cultures encourages.

Jacob Mincer focuses on the role of education in economic growth and its effects on wage structure in the labor market. Econometricians, he points out, frequently interpret changes in the quality of education in terms of increases or decreases in the productivity of schooling with resulting changes in the productivity of the market economy. He challenges this approach by reminding us that the quality of education is also a deep cultural concern in our society. He considers the sticky concepts of over and undereducation in this light.

Lauren Resnick emphasizes a new challenge to educational research. In the past, education solved many problems by serving individuals who learned quickly and by weeding out those who did not. We are now committed to the education of all children, including large, previously unschooled segments of society. The spectrum of backgrounds and of abilities represented in our classrooms requires that attention be devoted to the quality of education for a diverse population. Older principles of instruction are not adequate to this task and educational methods based on recent scientific advances must be developed that are relevant to broad-based demands for educational achievement.

Lee Shulman points to the utility of research on teaching and learning for the improvement of pedagogical practice and teacher preparation. He suggests that a common theoretical language is being forged which links our understanding of the cognitive functioning of pupils in the course of learning with the pedagogical activities of classroom instruction. This work is yielding an increase in our appreciation of the intricacies of the teaching task and is producing the extensive body of knowledge required for responsible teaching performance.

**Our schools will move beyond risk to obsolescence if the education profession fails to receive the research support that should underlie one of the most critical functions of an advancing society.**

In summary, this report should make clear that efforts to realize our aspirations for change in our educational systems, as well as in the social surroundings and public attitudes that aid school reforms, can be greatly assisted through research. With funds forthcoming, with promising scientists and scholars attracted into the fields of educational inquiry, and with teachers inspired by professional standards linked to advances in knowledge, we can effect productive, enduring changes. Our schools and the teaching profession generally should not be content with minimal nourishment from advances in science and technology. The recent calls for reform have made acceptance of this state of affairs intolerable, and developments in the disciplines and sciences that can contribute to educational improvement make the lack of support from science no longer necessary. The knowledge and the methods for obtaining new knowledge that are available through the work of a growing number of adept educational researchers can significantly benefit the design of policies and practices for education. Our schools will move beyond risk to obsolescence if the education profession fails to receive the research support that should underlie one of the most critical functions of an advancing society.

**References**

Hilgard, Ernest R. 1964. A perspective on the relationship between learning theory and educational practices. In *Theories of learning and instruction: The sixty-third yearbook of the National Society for the Study of Education, Part 1,* ed. Ernest R. Hilgard, 402-415, Chicago: University of Chicago Press.

National Science Foundation. The National Science Board Commission on Precollege Education in Mathematics, Science and Technology. 1983. *Educating Americans for the 21st century.* Washington, D.C.: U.S. Government Printing Office.

The Twentieth Century Fund. 1983. *Report of the Twentieth Century Fund Task Force on Federal Elementary and Secondary Education Policy.* New York: Twentieth Century Fund, Inc.

U.S. Department of Education. The National Commission on Excellence in Education. April 1983. *A nation at risk: The imperative for educational reform.* Washington, D.C.: U.S. Government Printing Office.

# Educational Research: Reflections on the Past

John B. Carroll

*John B. Carroll is William R. Kenan, Jr., Professor of Psychology Emeritus at the University of North Carolina at Chapel Hill, where he was Director of the L. L. Thurstone Psychometric Laboratory from 1974 to 1979. From 1949 to 1967 he was on the faculty of the Harvard Graduate School of Education and served as Director of the Laboratory for Research on Instruction there. From 1967 to 1974 he was Senior Research Psychologist at Educational Testing Service in Princeton. His research and writing have covered many fields, mainly substantive and methodological research in psychometrics and psychological testing, and research on aptitude and achievement testing in foreign language instruction.*

A perennial question has been whether educational research has had, or can have, any useful impact on what goes on in educational institutions – schools, colleges, universities, etc. – and in those many sectors of society that influence people's learning – the family, the media, etc. Three further focusings of the question ask whether, even if educational research *can* have some useful impact, it can be worthwhile and "cost-effective," whether there is now any more need for educational research since (as some may claim) "we know all the answers that can be found," and how educational research can best be conducted – where, by whom, and with what type of support. I cannot pretend to have final answers to these questions, though I certainly have opinions on all of them, but I can offer some reflections based on my experience with educational research over the past thirty years or so. I can also refer the reader to a most impressive and comprehensive treatment of "how research has changed American schools" recently published by a leading writer on educational research (Travers, 1983).

I shall be looking mostly on the bright side. Some twenty years ago (Carroll, 1963a) I was asked to state what I thought were the solid achievements of educational research, or more particularly, of educational psychology – which of course constitutes a large segment of educational research. Without much difficulty, I named a series of studies or findings that I thought significant, but as I did so, I realized that one of the reasons that educational psychology seems to be dealing with "the obvious" is that many consequences of research in educational psychology have become so much a part of our educational system that we tend to take them for granted as always having existed. My list started with Edward L. Thorndike and Robert S. Woodworth's overturning of the formal discipline theory (1901); it continued with Thorndike's Law of Effect (1913) – now embodied, through the work of B. F. Skinner (1954), in various forms of instructional technology and "behavior modification." It cited still other studies roughly in the domain of the management of school learning, such as Arthur I. Gates' demonstration of the principle of active learning (1917), William A. Brownell's studies of meaningful learning vs. drill in arithmetic (Brownell, 1947; Brownell & Chazel, 1925), and Kurt Lewin, Ronald Lippett, and Ralph K. White's studies of styles of classroom leadership (1939). In matters of individual differences as they affect school learning, it cited the development of the intelligence scale by Alfred Binet and Theodore Simon (1905), the invention of the objective test and the spread of psychometrics, Lewis M. Terman's studies of giftedness (Terman, 1925; Terman & Oden, 1947, 1959), the work of Charles Spearman (1927) and of L. L. Thurstone (1938) on theories of intelligence and cognitive ability, and Hugh Hartshorne and Mark A. May's overthrowing of the concept of the generality of character (1928). Finally it mentioned several outstanding events in the progress of curriculum and policy research methodology, such as W. S. Learned and B. D. Wood's "Pennsylvania Study" (1938), Ralph Tyler's Eight-Year Study of progressive education (as recounted by Aiken, 1942, and Chamberlin et al., 1942), and the introduction of

**. . . many consequences of research in educational psychology have become so much a part of our educational system that we tend to take them for granted as always having existed.**

sophisticated experimental design in educational research – as illustrated, for example, by Walter Deemer and Phillip Rulon's study (1942) of methods of teaching shorthand.

Those familiar with the history of educational research will recognize that the basic findings of these studies are, if not universally accepted or fully acted upon by all teachers, well woven into the foundations of modern educational theory. What is more, they are in most instances well woven into contemporary educational practice. The schools would be quite different today, I think, if the researches I have mentioned had never been carried out.

**. . . the schools are well served by knowing that formal discipline of the mind is not as effective as was once thought, that positive reward is one of the best motivators, that active learning is more productive than passive learning, that there are detectable and influential individual differences in pupils' abilities, that there is really no such thing as "good character," and that learning involves understanding as much as it may require practice and exercise.**

Some might argue, of course, that the schools would be better off today without these researches, but such a claim would be exceedingly difficult to support. Admittedly, my denial of any such claim might also be difficult to support, but I believe most people would agree that the schools are well served by knowing that formal discipline of the mind is not as effective as was once thought, that positive reward is one of the best motivators, that active learning is more productive than passive learning, that there are detectable and influential individual differences in pupils' abilities, that there is really no such thing as "good character," and that learning involves understanding as much as it may require practice and exercise.

Such was my list in 1963; I hesitate to spell out a similar list of accomplishments for the last twenty years because I fear that I would unjustly leave out many important discoveries and findings. For all its difficulties and confoundings (as discussed for example in a work edited by Frederick Mosteller and Daniel Moynihan, 1972), one can hardly doubt that the "Coleman Report," *Equality of Educational Opportunity* (Coleman et al., 1966) has had a substantial impact on progress toward school integration and improvement of educational opportunities for minority groups. In regard to areas with which I am most familiar, I would claim that Benjamin S. Bloom's concept of "mastery learning" (Bloom, 1974, 1976), the recognition of the importance of usefully invested time in learning, and the evolution of a solid psychology of the reading process (Gibson & Levin, 1975) have had widespread positive influences on teaching and learning. "Programmed instruction," once considered an uncertain solution for the teaching/learning process, has had a rebirth – fortunately, perhaps, under other names – through the advent of microcomputer technology. All these developments have come about only because instructional psychology has now become a field of serious and devoted activity (Resnick, 1981). Finally I would mention the important and useful effect of the development of criterion-referenced testing (see, for example, Berk, 1980).

On various occasions in the past I have published (Carroll, 1960, 1961) some rather bleak assessments of the state of educational research, identifying problem areas that I thought had not been adequately pursued or treated. I complained about the lack of experimentation in many areas of education,

**During the past twenty years . . . I believe many . . . deficiencies have been remedied. We now have much more information, to be found in reams of educational research studies, about the conditions of learning, the management of educational differences, the motivation of students, and other matters that I have mentioned.**

the undue emphasis often given to measurement and the statistical properties of measurements, the absence of good information about the relation of learning rates to abilities and aptitudes, the paucity of adequate theories about motivating students, and inadequate attention to how laws of learning can be applied in teaching. During the past twenty years, however, I believe many of these deficiencies have been remedied. We now have much more information, to be found in reams of educational research studies, about the conditions of learning, the management of educational differences, the motivation of students, and other matters that I have mentioned. I will not say that our knowledge is as complete as might be desired – I think, for example, that there is much still to be learned about the role of individual differences in learning, and I am currently engaged in a survey of what we know and do not know about individual differences in cognitive abilities. I am inclined, however, to take the cheerful view that the educational research of the past twenty years provides us with the information on which more effective education can be based. Whether that information is being well taken advantage of, or even whether it exists in an accessible, useful form is another story – one that needs discussion and continued attention.

In thinking about the ways educational research has influenced education, I've noted that there seems to be a characteristic course of events, to wit:

(1) A problem is perceived by teachers, subject-matter specialists, or other educational authorities. At this point educational researchers stand somewhat in the background, although sometimes problems are perceived most fully by researchers in advance of their being perceived by practitioners.

(2) A typically oversimplified and unrefined solution, model, or idea for meeting the problem is proposed somewhere in the educational community – sometimes among practitioners, but more often by educational researchers or by people who might be called "educational commentators" – people like James B. Conant, Rudolf Flesch, Charles Silberman, or "back-to-basics" proponents. The solution is inevitably crude because there is inadequate research to indicate how it should be refined.

(3) This oversimplified solution is disseminated throughout the community of practitioners. Dissemination is usually rather gradual, but it can be rapid if it is "*media*ted" – that is, promoted through educational or public media, or propelled by educational activists. The idea or solution is widely adopted, at least in lipservice, in the educational community, with varying degrees of competence, thoroughness, and success.

(4) Educational researchers step in at this point; through their researches, which inevitably require considerable stretches of time, they are gradually able to refine the solution, model, or idea – defining its qualifications and the limits of its application.

(5) The results of educational research gradually seep into educational practice – through teacher-training activities, the publication of findings in media addressed to practitioners, the preparation of textbooks for use in schools, etc. The process is so gradual that it can scarcely be readily perceived; nevertheless it does in fact take place, as could be documented by careful historical work.

**We now have a much better approach to the teaching of reading – well refined by several generations of researchers – and the refinements are now reaching the stage where they are affecting educational practice far and wide, even showing up, perhaps, in national statistics on the achievements of young readers.**

The history of the "model of school learning" that I proposed in an article in *Teachers College Record* (Carroll, 1963b) provides one illustration. As an educational psychologist I perceived a problem having to do with the relation between time allowed for student learning and the capacities of students to learn in the time allowed. It seemed to me that many students were not allowed the time they needed for learning, and that the time they needed was a function of individual characteristics. Some of my ideas – still in a rather crude form – were taken up in Benjamin S. Bloom's "mastery learning" proposals. What got disseminated to the educational community was the rather simple idea that time is important for learning. It has taken a considerable period for Bloom and others to demonstrate that matters are not quite that simple – that it's what happens *during* the time allowed for learning that is important, also that student aptitudes for learning can possibly be improved through proper use of time. Only now are these refinements reaching the community of practitioners. One other illustration can be drawn from the history of the psychology of reading and its teaching – where the "oversimplified solution" proposed by various educational commentators has almost always been to "teach phonics." We now have a much better approach to the teaching of reading – well refined by several generations of researchers – and the refinements are now reaching the stage where they are affecting educational practice far and wide, even showing up, perhaps, in national statistics on the achievements of young readers.

In sum, we can make two principal observations about educational research as it has progressed in this century. First, some of the research of the first fifty years or so has had important impacts on school practice – impacts that have not been fully appreciated either by practitioners or by the public. But second, in the past twenty years educational research has, so to speak, come of age and is now able to address in a definitive way the issues in teaching and school learning that are of greatest concern to educators and the public.

**References**

Aiken, W. M. 1942. *Adventure in American education.* Vol. 1, *The story of the Eight Year Study.* New York: Harper.

Berk, R. A. 1980. *Criterion-referenced measurement: The state of the art.* Baltimore: Johns Hopkins University Press.

Binet, A., and T. Simon. 1905. Methodes nouvelles pour le diagnostic du niveau intellectuel des anormaux. *Annee Psychologique,* 11:191-336.

Bloom, B. S. 1974. Time and learning. *American Psychologist,* 29:682-688.

Bloom, B. S. 1976. *Human characteristics and school learning.* New York: McGraw-Hill.

Brownell, W. B. 1947. The place of meaning in the teaching of arithmetic. *Elementary School Journal,* 47:257-265.

Brownell, W. B., and C. B. Chazal. 1925. The effects of premature drill in third-grade arithmetic. *Journal of Educational Research*, 29:17-28.

Carroll, J. B. 1960. Research in education: Where do we stand? *Harvard Graduate School of Education Association Bulletin,* 5(3): 2-7.

Carroll, J. B. 1961. Neglected areas in educational research. *Phi Delta Kappan,* 42:339-343.

Carroll, J. B. 1963a. Educational psychology: Achievements and prospects. Paper presented at the meeting of the National Society of College Teachers of Education, Chicago, February.

Carroll, J. B. 1963b. A model of school learning. *Teachers College Record,* 64:723-733.

Chamberlin, D., E. S. Chamberlin, N. E. Drought, and W. E. Scott. 1942. *Adventure in American education,* Vol. 4, *Did they succeed in college?* New York: Harper.

Coleman, J. S., E. Q. Campbell, C. J. Hobson, J. McPartland, A. M. Mood, F. D. Weinfeld, and R. L. York. 1966. *Equality of educational opportunity.* Washington, D.C.: U.S. Office of Education.

Deemer, W. L., and P. J. Rulon. 1942. *An experimental comparison of two shorthand systems.* Cambridge: Harvard University Press.

Gates, A. I. 1917. Recitation as a factor in memorizing. *Archives of Psychology,* No. 40.

Gibson, E. J., and H. Levin. 1975. *The psychology of reading.* Cambridge: M.I.T. Press.

Hartshorne, H., and M. A. May. 1928. *Studies in deceit.* New York: Macmillan.

Learned, W. S., and B. D. Wood. 1938. *The student and his knowledge.* New York: Carnegie Foundation for the Advancement of Teaching, Bulletin No. 29.

Lewin, K., R. O. Lippett, and R. K. White. 1939. Patterns of aggressive behavior in experimentally created "social climates." *Journal of Social Psychology,* 10:271-299.

Mosteller, F., and D. P. Moynihan. 1972. *On equality of educational opportunity.* New York: Vintage.

Resnick, L. B. 1981. Instructional psychology. *Annual Review of Psychology,* 32:659-704.

Skinner, B. F. 1954. The science of learning and the art of teaching. *Harvard Educational Review,* 24:86-97.

Spearman, C. 1927. *The abilities of man: Their nature and measurement.* New York: Macmillan.

Terman, L. M. 1925. *Genetic studies of genius.* Vol. 1, *Mental and physical traits of a thousand gifted children.* Stanford, CA: Stanford University Press.

Terman, L. M., and M. H. Oden. 1947. *Genetic studies of genius.* Vol. 4, *The gifted child grows up.* Stanford, CA: Stanford University Press.

Terman, L. M, and M. H. Oden. 1959. *Genetic studies of genius.* Vol. 5, *The gifted group at mid-life.* Stanford, CA: Stanford University Press.

Thorndike, E. L. 1913. *Educational psychology.* Vol. 2, *The psychology of learning.* New York: Teachers College.

Thorndike, E. L., and R. S. Woodworth. 1901. The influence of improvement in one mental function upon the efficiency of other functions. *Psychological Review,* 8:247-261, 384-395, 553-564.

Thurstone, L. L. 1938. Primary mental abilities. *Psychometric Monograph,* No. 1.

Travers, R. M. W. 1983. *How research has changed American schools: A history from 1840 to the present.* Kalamazoo, MI: Mythos Press.

# How Might Policy Research in Education Be Better Carried Out?

James S. Coleman

*James S. Coleman was born in Bedford, Indiana on May 12, 1926. He received his bachelor's degree from Purdue University in 1949, and was awarded a Ph.D. from Columbia University in 1955. In 1970 he was made an honorary Doctor of Laws by Purdue University. From 1959 to 1973 he was an associate professor in the Department of Social Relations at Johns Hopkins University. Since 1973 he has been a professor of sociology at the University of Chicago. Professor Coleman is a member of the American Sociological Association and the Sociological Research Association.*

I will begin from the perspective of a sociologist. Beginning this way, I note first the most prominent fact about research in educational sociology, placing it apart from much of sociological research: The research is inherently policy-relevant. The contrast with research in social stratification – perhaps the most popular area of research in sociology – is especially striking. The study of social stratification examines how a person's social position (especially the person's occupation) is affected by various other characteristics – particularly father's occupation, own earlier occupation, and education. Sometimes these investigations are separated into two categories. The first is intergenerational mobility: How does the son's occupation differ from the father's, or less frequently, how does the daughter's differ from the mother's? (The second of these is, currently, far more interesting than the first, which showed the sharpest changes half a century or more ago.) The second category is intragenerational mobility, or how a person's occupation changes from one point to a later point in life.

What is most interesting to me about this area of research is just why sociologists direct so much attention to it, at least as its problems are currently defined. I suppose the answer lies in social structures of the past, first the period when the particular estate into which they had been born determined persons' lives and then the next stage, which might be described as Marx's stage, in which there was a largely hereditary proletariat. At least in part because of these long-enduring social structures, which were not only stratified, but also largely fixed in their stratification, sociologists have found the study of social stratification a fascinating area of investigation.

**. . . note, first, the most prominent fact about research in educational sociology, placing it apart from much of sociological research: The research is inherently policy-relevant.**

I have never found it so. The reason is simple. As commonly defined and carried out, it leads nowhere. Harold Lasswell gave the label "policy sciences" to the social sciences. But the study of social stratification, as commonly pursued, is almost wholly unrelated to policy. It addresses questions that are largely irrelevant to policy. The study of social stratification, as commonly carried out in sociology, remains, in my estimation, principally a means by which assistant professors become associate professors, and associate professors become full professors. This, of course, is too strong a statement in two ways: This research currently does provide information about social processes that can be of long-term value for social theory, and even for social policy. In addition, there are problems related to social stratification, for example those having to do with unemployment and with changes in the occupational structure, that are directly relevant to policy. A somewhat differently oriented body of work in social stratification would be of more immediate relevance for social policy. Nevertheless, the broad outlines of the field as currently defined in the discipline have, I believe, the properties I have described.

The general irrelevance to social policy is true of much research in sociology. It may lead to important new knowledge for the discipline. But it leads nowhere for those outside the discipline. Not so with the study of

**Practitioners must make policy decisions because the educational enterprise is based on formal organization, and thus on policy choices in educational organizations. This necessity for regularly shaping policy creates a body of consumers for social research in education.**

educational institutions. Here, the outcome of sociological research may make a difference. It may for a simple reason: The factors whose effects on educational outcomes are investigated are subject to policy control. If it is found that a certain way of organizing classroom activities increases children's interest in learning, or increases their achievement, then superintendents and principals and teachers may begin to reorganize classrooms. If it is found that tracking or grouping has certain effects on either the average level of achievement in schools or the variation in achievement between the brighter and less bright students, this affects the practice of tracking or grouping. And so on. The impact of social research on educational policy is not trivial. Sociologists may sometimes feel that educational practitioners pay too little attention to their research; yet a case can be made that practitioners pay too much attention. Practitioners must make policy decisions because the educational enterprise is based on formal organization, and thus on policy choices in educational organizations. This necessity for regularly shaping policy creates a body of consumers for social research in education.

**. . . the mechanism for implementing research results is often right there, part of the very structure of education itself, waiting in the wings for information that will promote its interests.**

The question of how and why social research in education has an impact on practice, or fails to, is not answered in a few sentences. But the answer does include the conception of *interested parties*, in somewhat the following way: If the results of the research support the position of interested parties who are able to press their interests, those results will become part of their agenda. It is not always true that social research findings are of interest to such parties, nor is it always true that when research implications *are* of interest to some party, they will be able to overcome opposition to those implications. Nevertheless, the mechanism for implementing research results is often right there, part of the very structure of education itself, waiting in the wings for information that will promote its interests.

Now let me shift roles, from that of the sociologist to that of the educational policymaker, the educational practitioner, or the ordinary citizen who has expressed opinions about education and wants to be kept informed. These are, of course, some of the interested parties that I just mentioned. From this perspective, I must ask the sociologist: What right do you, sociologist, have to such an impact? So long as your concern is only with those matters like social stratification that you have claimed have little effect on the functioning of society, then you have the right to study anything you want to, and to announce it as you wish. But when your investigations have the potential for causing change, then I have a right to question how you go about your work.

From this perspective – that of the policymaker, practitioner, or citizen – I find social research in education quite unsatisfactory on several counts. First, it seems to address itself not to matters that concern me, but rather to questions that are arrived at through some sort of mysterious process. I may not know that the research idea began in the fertile brain of an official at NIE or NCES, then made its way into reality through an RFP and a set of proposals

**Were I a teacher in the inner city, in the early 1970s, I might wonder . . . why no one studies attendance, since it would be apparent to me that one of the reasons I am less successful in teaching is that, on any given day, a large number of my pupils are not in class.**

from eager prospective investigators – or that it sprang from the mind of a sociologist trying to discover a way to have an impact on sociology, or to make tenure, or to get an article published in the *American Journal of Sociology*. All I would know is that the research fails to focus on issues of importance to me in trying to run the schools, or teach, or see my children get a good education. Were I a teacher in the inner city, in the early 1970s, I might wonder (as at least one did in correspondence) why no one studies attendance, since it would be apparent to me that one of the reasons I am less successful in teaching is that, on any given day, a large number of my pupils are not in class. The absence of research on attendance and the causes of its variation is just one of many examples in which my interests are not even addressed in educational research. Somehow, the mysterious process of formulating research passes these questions by.

The research is unsatisfactory also on a second count. Social researchers in education are apparently unwilling to commit themselves. For example, as a practitioner, I noticed, in the late 1960s and early 1970s, something called "Planned Variations" in education, which was based on Follow Through programs, beginning where Head Start left off. The program provided for striking and wide variations in the modes of instruction, variations ranging all the way from open classrooms to behavior modification. Yet when I read the results of the research on these programs to learn what happened, I found that, not only did the researchers fail to investigate matters important to me – such as effects on attendance, or how much implementations cost, or the effect on teacher moral – but that on the single matter they did address – student achievement – there was extraordinary irresolution and equivocation. These variations *must* have made a difference, yet there was no way I could find that out from the research reports. The researchers appeared to be afraid to commit themselves.

Finally, I find research unsatisfactory in a third way as well. When there are research results, when the sociologist *does* make a definite statement, almost instantly another social scientist refutes the conclusion. I find myself lost in the maze of charges and countercharges, claims and counterclaims. There appears to be no authoritative voice in social science to tell me that a claim is true or false. I recognize the inherent problems in research; but this continuing inability to agree, even on a few results, is from my point of view wholly unsatisfactory. It seems to me that it can hardly be necessary.

Now let me step out of the roles of sociological researcher or educational practitioner, and take on that, if you will, of social or political philosopher. In this role, I will ask how something may be done about these three unsatisfactory aspects of educational research. Essentially, I will suggest that there are two kinds of remedies, neither dealing directly with the *execution* of social research in education, but one concerning the activities prior to the research, and one concerning the way its findings are organized and then made public.

The first remedy I will call pluralistic policy research design. It is research design that does not attempt to be "value free," as is the common prescription in social science disciplines, but attempts rather to make use of the variety of interests and values in the design process itself. It is based on the premise that research that is publicly funded (as most research in education is) ought to address questions of interest to all the parties who have legitimate interests in education. Research in education should attend to interests of children from various backgrounds and with various prospective destinations; consumers of educational products, including employers, as well as subsequent educational institutions; parents, who have various goals for their children's schooling; and the providers of education, such as teachers, principals, and superintendents. Researchers have no responsibility to give *answers* that support these groups' particular interests, but they have a responsibility to address the *questions* of concern to them.

**. . . preliminary work should be directed first to discovery of the structure of interests surrounding the particular aspect of oducation to be studied, then to discovery of the kind of information that is relevant to each of these interests.**

To do this involves sociopolitical investigation prior to the educational research itself. Whether carried out by the funding agency or the research investigator, this preliminary work should be directed first to discovery of the structure of interests surrounding the particular aspect of education to be studied, then to discovery of the kind of information that is relevant to each of these interests. This information should provide the basis for the research design – a design that can give information relevant to the spectrum of interests described. I will not go into questions of just how this might be done, a matter I have written about in a report for the National Center for Educational Statistics, entitled "Policy Issues and Research Design" (Coleman et al., 1979), but the task is not an impossible one, once its value is recognized and it is regarded as the proper first step in the design of educational policy research. What is largely missing is the recognition and acceptance of this as the proper way to initiate the design.

Now, still in the role of political philosopher, I will turn to the second remedy, concerned with research organization and dissemination of results. To do so, I must draw a contrast between research intended simply to advance knowledge in a discipline, and research intended to be relevant to policy. To advance knowledge within a discipline, competition leads investigators to challenge prior research results in subsequent research. This provides, through a sequential "survival" process, a means of arriving at robust results. However, in research intended to be relevant for policy, this sequential process is insufficient. Various proposals for institutions that will substitute for this process have been made. The most prominent of these is the "science court" ("The Science Court Experiment: An Interim Report," 1976). These proposals make it clear that the problem pervades policy-relevant research, in natural science, as well as in social science. Errors of two kinds are possible, and both can be found in past educational research. One is the release of research results without sufficient critical review to assure their correctness and robustness. Even if the results ultimately prove correct, the lack of such

critical or adversarial review generates public controversy, which casts doubt on results and reduces the credibility of educational research.

The second kind of error is of the opposite kind. It arises from the investigator anticipating controversy, and eliminating from the results (or even from investigation) anything potentially controversial – or hedging conclusions so extensively that no policy implications remain. As a result, research that is potentially important for policy is obscured or lost.

**. . . errors result from the lack of a dialectical or adversarial process as an intrinsic part of the research investigation itself . . . . Planning for it would constitute an important step toward presenting research results that are highly credible . . . .**

Both of these types of errors result from the lack of a dialectical or adversarial process as an intrinsic part of the research investigation itself. Yet such a process can be an intrinsic part of a research project. Planning for it would constitute an important step toward presenting research results that are highly credible, not because they are trivial or watered down, but because they have survived procedures designed to test them.

The processes for achieving this will differ, depending on the kind of research at issue. To illustrate, I will describe how it could be carried out in a simple case, a case in which the interests surrounding the issue have been identified, and data on the information relevant to them have been gathered. What remains is the analysis and the publication of results.

Although this is a relatively simple case, it is by no means trivial. There exist two large data bases collected under the auspices of the National Center for Educational Statistics, representative samples of high school students (seniors in 1972, sophomores and seniors in 1980); both have been followed in subsequent data collection. NCES recently issued an RFP for an analysis addressing several important questions using these data bases. Organization of the analyses that I describe below would be appropriate for a project funded under such an RFP. There are three basic procedures:

(1) For problem areas likely to be highly controversial, two investigators whose previous work has led to opposing conclusions would conduct independent analyses. These I will term "competitive analyses."

(2) All results, including those from the competitive analyses, should be subject to critical review by a committee of researchers familiar with the data. The committee should meet twice with the researchers responsible for the competing analyses. Before the first meeting, the committee would be provided with preliminary results, and given the opportunity to request a limited number of analyses prepared for their own use, to their own specifications. This meeting would necessarily be somewhat adversarial, but less so than if the results were final and had been made public. In addition to individual positions to be defended, there would be a collective goal of arriving at a fully defensible product.

The second meeting would proceed in much the same way as the first, but in the interim the principal analysts and members of the review committee would be in contact. In addition, between the first and second meetings, the analysts responsible for the pair of competitive analyses would be free to communicate, and possibly to resolve their principal differences.

(3) Report authorship would reflect the two procedures I have described. The reports in those areas subject to competitive analyses would be in two parts, based on the two analyses begun independently. Both investigators would be principal authors and the report should reflect their efforts to resolve differences – a process aided by the committee procedure. However, it is quite possible that important differences would remain.

For each report, the members of the review committee would be listed as secondary authors, with an indication of the supplementary and complementary role they played. An opportunity would be provided in appendices for the members' presentations of interpretations differing from those in the body of the report. If the procedures described in (1) and (2) above work well, there should not be a large number of such statements. But neither should there be none at all, if the reports present results that have important implications for policy.

As indicated, the process for organization of the research and publication of the results will differ, depending on the type of research. The specific mechanisms of competitive analysis, committee review, and joint publication would not apply directly to all types of research. However, they illustrate how such processes can become part of the project organization.

**The two remedies I have suggested – pluralistic policy research design and competitive analyses with critical review – do not, of course, eliminate what are properly regarded as unsatisfactory aspects of social research concerned with education. They are steps in that direction, but only steps.**

The two remedies I have suggested – pluralistic policy research design and competitive analyses with critical review – do not, of course, eliminate what are properly regarded as unsatisfactory aspects of social research concerned with education. They are steps in that direction, but only steps. What is important, I believe, is to recognize the problems; to recognize that the methods and the practices in initiation, design, and publication of research results that have evolved for academic disciplines are *not* fully satisfactory for social policy research in education. Once we have fully recognized this, then the way is prepared for developing practices that will raise the grade that social policy research in education gets from the teacher, or the principal, or the parent, from "unsatisfactory" to "satisfactory."

**References**

Coleman, J., V. Bartot, N. Lewin-Epstein, and L. Olson. October 1979. *Policy Issues and Research Design* (Contract No. 300-78-0208). Report to National Center for Educational Statistics by the National Opinion Research Center.

"The Science Court Experiment: An Interim Report." *Science*, 193:653-656.

# "The Value of Research and Disciplined Inquiry to the Improvement of Education": Impressions of a Nonresearcher

Harold Howe II

*Harold Howe II is a senior lecturer at Harvard Graduate School of Education. He is a trustee of Teachers College, Columbia University, and chairs the boards of both the Institute of Educational Leadership in Washington, D.C., and the Center for Effective Philanthropy. Howe also participates as a board member on the Council for Advancement of Private Higher Education, the Business Council of Effective Literacy, Inc., and the Hispanic Policy Development Project in New York. As co-chairman of the National Coalition of Advocates for Students' Board of Inquiry, Howe is orchestrating a study of the adequacy with which recent school studies address the needs of disadvantaged students. In addition to advising several foundations and committees on their educational interests, Mr. Howe has been busy writing articles, making speeches, and providing testimony in Congress regarding policy issues in education.*

The first part of the title of these remarks is the phrase Robert Glaser used in his letter asking me to contribute to this discussion; the second part indicates my lack of bias on this subject. I can lay claim to having supported considerable research with government and foundation money, as well as numerous efforts to contend with the unexpected results of such research, but I have never done any research and do not intend to start now. I do, however, have five points to make on the topic of educational research, and after mentioning them, I will talk about each in turn.

(1) Most people think educational research is of little use.

(2) It is easier to demonstrate the value of "disciplined inquiry" than it is the value of educational research narrowly defined.

(3) Commissions to study education's ills are occasionally useful, but not often enough to warrant giving them encouragement.

(4) Research on education has important long-term effects, but these are hard to explain to the people who make decisions about education.

(5) Because of the long-term nature of the effects of research on education, it is difficult (perhaps impossible) to convince political authorities to support it.

On the first point, people respect research in inverse proportion to their familiarity with the field in which it is done. So they tend to be enthusiastic about research on the frontiers of biological science, on the origins of the universe, or even on the activities of prehistoric man. But when research delves into matters people encounter in their daily lives – into such subjects as the family, the effects of television, consumer behavior, or education – they feel free to ignore it, or to regard its findings as either wrong or not applicable to them.

**. . . when research delves into matters people encounter in their daily lives – into such subjects as the family, the effects of television, consumer behavior, or education – they feel free to ignore it, or to regard its findings as either wrong or not applicable to them.**

This generalization, which I will call Howe's first law of the value of research, applies particularly to successful people, who find themselves in the leadership roles in society – government officials, college trustees, school board members, and corporate officers. Almost by instinct, they know how education should be conducted; the kind of education that produced them is what the rest of the people need.

This simplistic point does not wholly account for the lack of enthusiasm for educational research, but it accounts for much of it.

On point two, I am glad that Bob Glaser included the phrase "disciplined inquiry" in his letter, suggesting that it differs from research. In my view, the distinction is important. To produce generalizable results in the light of some theoretical formulation, such inquiry does not have to meet all the requirements of true research for proper design, methodology, and data gathering.

These aspects of research may protect its integrity, but they may also impose the penalty of curtailing its current usefulness. In addition, they sometimes have the effect of making reports on education deadly dull to read, a fact I have rediscovered through acquiring doctoral advisees at Harvard.

**. . . I called together a group that included principals, a superintendent, former teachers, and specialists in curricular affairs. I asked them what help they received in their work from research about education. Their reply was "little or none."**

Several weeks ago, as part of an effort to shape work at the Harvard Graduate School of Education more closely around what transpires in schools, I called together a group that included principals, a superintendent, former teachers, and specialists in curricular affairs. I asked them what help they received in their work from research about education. Their reply was "little or none." As the conversation continued, it turned in two directions. One was discussion of the possibility of encouraging research that would be useful to schools, partly because it was done in them; the other was about "disciplined inquiry," although that term was not used. These educators were interested in scholarly activity, other than formal research, as potentially more useful to schools.

When I reported their conversations to some colleagues at Harvard, I received a memo from a senior lecturer, who knows about and does research. It reads, in part, as follows:

> If school people want solutions to problems, research is neither an efficient nor necessarily an effective route to take. Research takes time, it costs money, and even more importantly, it rarely leads to authoritative "answers." To hold out the promise to school people that research will solve their problems is, I believe, misleading and probably counterproductive.
>
> Research is very good at many things: identifying issues and alternative perceptions of issues; defining the extent of problems; providing new perspectives on how to approach problems; examining the effects of current practices; evaluating the effects of innovations; revealing diversity of opinion about the relative salience of problems, and about future actions; identifying the behaviors of people or groups in the institution under a variety of conditions; and so on. Many of these suggest new ways to proceed, but research does not unequivocally demonstrate that doing A is the right solution.
>
> The greatest benefit of research on schools is to give people alternative ways of thinking about their situation and critical insights into why things happen as they do. But specifying remedies or innovation – in any concrete case – depends on independent and creative thought, experience and wisdom. To lead people to expect research to do that part of the job is, I think, a disservice to school people and to research.

**. . . disciplined inquiry . . . may not always be generalizable, but it does have the advantage of being relevant to a particular situation and the people who must deal with it.**

I would add that disciplined inquiry, by contrast, may not always be generalizable, but it does have the advantage of being relevant to a particular situation and the people who must deal with it. And its products often make better reading and reach a wider audience. I would classify James Conant's exploration of American high schools as disciplined inquiry.

The issue of disciplined inquiry – its potential value – comes up again in my third part. A particular mode of *disciplined inquiry* is the committee, commission, or panel appointed to examine some problem and tell educators, or the government, or the general public what to do. Typically, this process involves a group of distinguished and busy people, who don't know much about the subject, assisted by a staff that does. But there are all sorts of variations. It is interesting to note that James Conant did not have a commission. He just looked around, on his own, with the assistance of a few young helpers, who were not primarily scholars, although they were reasonably intelligent.

My only purpose in bringing up such arrangements for the study of education is to suggest that the Academy not encourage them. There are so many enthusiasts for commissions, among them governors, foundations, the Congress, the national administration, unemployed ex-college presidents, and other authorities that encouragement is not required.

Furthermore, the arrival of the age of media hype has placed an intolerable burden on commission activities. There was a time when a commission could labor hard and long to produce a balanced and carefully qualified report, with recommendations on constructive changes. It could expect that careful reading, by thoughtful and responsible people, would start to move thought into action. Nowadays, however, a commission's work will go unheeded unless its report contains enough ginger to get it on the Today Show. So rhetoric tends to rise above substance in the competition for that scarcest of all commodities – TV time on a national network.

I can think of a fair number of commissions in the past fifteen years that cost pots of money and produced minimal results. I served on some of them and helped to start others. Reflecting on these sins, I sometimes wonder whether the money would not have been better spent on the thinking, research, and writing of carefully selected individuals. Perhaps a fit subject for research would be analysis of those factors that tend to make a commission's work useful. One of the most successful groups I know about served the Detroit Public Schools under the leadership of a merchandizing executive named Stanley Winkelman. That commission made an initial decision not to write a report. Its members agreed to find out what needed to be done and to see that it got done.

As to my fourth point, research's long-term effects and the problems of arguing for them, a strong case can be made that serious, long-term research on education has been powerfully significant in the improvement of American

**. . . serious, long-term research on education has been powerfully significant in the improvement of American schools. At its inception, such research usually cannot be presented as a school improvement effort, because the researcher isn't really sure what may be discovered.**

schools. At its inception, such research usually cannot be presented as a school improvement effort, because the researcher isn't really sure what may be discovered. Take, as illustration, work in these four areas:

(1) Over many years research on the physical, emotional, intellectual, and moral development of children has gained us a sound basis for more humane and effective dealing with all aspects of children's lives, in the home, in the school, and in the community. Most of the people who did this research did not begin with the idea of changing or improving schools. They were simply trying to understand children better by observing them scientifically. Yet, today, countless promising school practices can be traced to earlier work in child development.

(2) One of our colleagues in the National Academy of Education, Larry Cremin, has been busy for some years teaching us, in his multivolume series *American Education*, to think about education more comprehensively. His historical research has illuminated the breadth of our sources of education from prerevolutionary times and so has altered the meaning of the word education. When his work on more recent history appears, it will expand our definition further, producing profound changes in the way people think about education and ultimately in what they do about it. But when the commitment to start this research was made, all that could be said was that the history of American education would be re-examined.

(3) Another researcher, Robert Coles, has elected, over the past fifteen years, to talk with children and their families in an effort to understand and report on the values, motivations, and cultural influences that dominate their lives. He used his background in psychology and psychiatry to probe deeply and to elicit their feelings. Proving the utility of the work he has reported in several volumes is an elusive task, and yet anyone who has read those volumes can't help feeling an enhanced level of sensitivity to children that will affect all future dealings in their affairs.

(4) Two seminal books by scholars of education have changed our thinking about school finance in the United States. *Private Wealth and Public Education* by Jack Coons and colleagues, and *Rich Schools Poor Schools* by Arthur Wise, published in 1970 and 1968 respectively, resulted in further work by economists, political scientists, and sociologists. This entire body of research has had a very direct effect on concepts of equity for both students and taxpayers who support them. These concepts, in turn, have over a fifteen-year period influenced the actions of dozens of courts and legislatures, and launched important changes in the financing of public schools by states and localities. In this instance, there was probably a more immediate connection between the research and its results than in those cited above.

During the 1970s, the National Institute of Education made efforts to tune its programs, so that they might support research that promised long-term benefits and had largely been financed by private funds. It adopted a policy of

growing investment in basic or long-term research, as opposed to quick fixes that might satisfy the Congress. But this policy fizzled as budgets were cut and "practical" projects that promised immediate results were advanced. Decisions leading in this unconstructive direction were encouraged by the Regional Education Laboratories – a good idea run wild. They formed a political coalition, which effectively lobbied for all laboratories regardless of their merits. The result was the erosion of discretionary funding for long-term, fundamental research activities. This turn of events leads to my final point.

Our political process requires that our national leaders are elected every two, four, or six years. They must demonstrate their achievements in those time periods. Support for the examples of research I have cited could not have been justified in that time scale.

Doubt and worry plague our recent thinking about financial support for educational research. The view that the national government should support research is being challenged on at least three grounds: for reasons outlined above, government is unlikely to fund the best research; the past twenty years of government support for educational research have tended to produce shallow results; the present administration has twisted so radically the process both of making research grants and of determining what is to be supported that the National Institute of Education should be abandoned in the name of progress.

Reflecting again on my own sins, as well as those committed by others caught in this tangled web, I wonder if people who are genuinely concerned with seeing that we have effective education would not be helped by keeping in mind certain facts about research. The best research in education shares some of the attributes found in the best research in science; researchers who aren't sure what they are looking for often make the discoveries that will bring major changes. The National Science Foundation and the National Institute of Health have done a better job than NIE in keeping this in mind when confronted with short-term political thinking, and NIE's supporters should take a cue from them. The strongest case for national support for educational research will be made by those who do it best, and they have been less visible than one might expect in endeavoring to influence the politics of this subject. There is no one doing for educational research what Lewis Thomas is doing for research in biological and medical science.

**We should try harder than we have to explain to Congress what research can do and what it cannot; we should enlist more able researchers and fewer educational entrepreneurs in making that case . . . .**

My own view on these problems and possibilities is that we should not give up on federal support for research on education. We should try harder than we have to explain to Congress what research can do and what it cannot; we should enlist more able researchers and fewer educational entrepreneurs in making that case; and we should not become discouraged because an administration with radical views about the federal role in education has cut across the slow but useful growth of maturity in the National Institute of Education.

# The Value of Comparative and Developmental Research on Education

Robert A. LeVine

*Robert A. LeVine is Roy E. Larsen Professor of Education and Human Development and Professor of Anthropology at Harvard University. He has conducted research on parenthood, the family, and childhood environments in subsaharan Africa and Mexico. Currently, he is studying the effects of schooling on maternal behavior in developing countries, and during 1983-1984 conducted research in Mexico. Professor LeVine participated in the Harvard Project on Human Potential and is preparing a monograph on the cultural conditions of educational development. He served as Chairman of the Laboratory of Human Development at the Harvard Graduate School of Education from 1977 to 1980. From 1980 to 1983, he was Chairman of the Board of the Social Science Research Council.*

In a society that values formal education and commits substantial resources to it, educational research is not merely a necessary evaluation process; it's part of a larger effort to understand what is possible in the field of human affairs, providing a scientific basis for making choices among possible ends and means. In discussing how educational research into individual development and the comparison of cultures and nations contributes to this larger effort, I shall refer to three tasks of educational research: (1) defining and locating education in ongoing social processes, (2) defining and assessing the educability of the human individual, and (3) describing and explaining the intended and unintended consequences of educational policy. Developmental and comparative studies are increasingly involved in these tasks. For each one, I shall argue that specific research studies have not only added new knowledge but have challenged conventional wisdom and revised our thinking.

**. . . educational research is not merely a necessary evaluation process; it's part of a larger effort to understand what is possible in the field of human affairs, providing a scientific basis for making choices among possible ends and means.**

First, comparative research has altered our views of what education is and where it takes place. Anthropological studies broadened the concept of education from its identification with formal tuition in school classrooms by showing that peoples without schools organize their children's learning in other settings, using a variety of other methods, for culturally distinct goals. Education is thus a property of all human societies, and all social settings are potentially learning environments. Although this is now taken for granted, it represents a change of viewpoint occasioned by research findings.

A related finding from sociological research shows that in industrial societies with compulsory schooling, environmental factors in the home settings of children may determine how much they learn in school. This is well known in our own society, and it has recently been receiving public attention as a dimension contributing to the differences between achievement in Japan and the United States. The Japanese patterns of tutoring, either at home by mothers or in special schools, were described by Ezra Vogel (1962) and other American social scientists twenty years ago. Though it may not have affected American educational theory or practice at that time, it gave empirical support to the idea that education or academic training, even in the narrowest sense, may be located outside the school. A concept of education that encompassed the process of learning in societies without schools, as well as nonschool learning in societies with schools, was required by the evidence emerging from comparative and domestic research.

More recently, the work of Michael Cole (1973) and his colleagues in Liberia (Scribner & Cole, 1982) has shown that, just as education should not be equated with schooling, so school learning should not be equated with literacy. By comparing Liberians who have become literate without schooling with those who have gone to school, they have shown that the cognitive impact of schooling cannot be attributed to gaining the specific skills of reading and writing, but seems to be a function of socialization in the school setting.

Finally, evidence emerging from African and other non-Western peoples suggests that, in agrarian societies, education as cognitive development is often believed to take place primarily in adulthood, rather than in childhood. Insofar as the focus is on the reflectiveness that comes from adult experience, this belief is probably valid. Like the foregoing examples, this evidence presents a challenge to older and narrower definitions of education.

**. . . comparative research has played a part in redefining education . . . when in the lifespan it is possible, what it consists of, and how it is related to schooling. The result, over all, has been an expanded definition . . . .**

I have gone over this familiar ground to remind us that comparative research has played a part in redefining education in terms of where it occurs, when in the lifespan it is possible, what it consists of, and how it is related to schooling. The result, over all, has been an expanded definition that characterizes schooling as a particular social and intellectual experience, rather than the primary locus of human learning. The most important research issues, however, such as determining the nature of home and school influences on learning and performance, and the psychological processes involved in school and nonschool learning, have not been settled; they remain critical areas for further inquiry.

The second task mentioned above is defining and assessing human educability. Here it has been primarily developmental research that has challenged existing beliefs. Leaving aside the ongoing controversy between Piagetian structuralism, which posits age-related constraints on educability, and other developmental psychologies that cast doubt on the existence of such constraints, it is possible to identify some genuine breakthroughs. Research on human infancy, over the last twenty years, has shown with increasing precision that humans are more capable of perception and learning at earlier months in the first year of life than anyone had previously thought. The most important educational implications do not lie in accelerating the development of skills in response to parental wishes for precocious "superbabies." Rather, they lie in the knowledge that babies are acquiring interactive styles, emotional codes, and relational standards from their early environments, whether the parents know it or not. What we should do about infant educability is another question, but it is a reality that cannot be denied.

**This methodological breakthrough has led to research showing which cognitive and learning capacities improve, which remain stable, and which decline in the later years — providing the more refined picture of adult educability that is crucial for our rapidly aging population.**

Research into the educability of the middle-aged and elderly has produced a similarly dramatic revision. Cognitive decline in the later years was an accepted phenomenon, until Warner Schaie showed that much of the evidence was based on cross-sectional samples in which persons of less formal education and cultural sophistication represented the older age points. With longitudinal testing later of the same persons, increased age was not accompanied by decline. This methodological breakthrough has led to research showing which cognitive and learning capacities improve, which remain stable, and which decline in the later years — providing the more refined picture of adult educability that is crucial for our rapidly aging population.

Finally, developmental investigators have shown how much individual differences in children's educability depend on controllable social and psychological factors. Here I think of Bloom's (1981) studies of exceptional achievers, showing the family support involved, and of Dweck's (1983) studies of self-concept among children differing in academic performance. This research, like Hiroshi Azuma and Robert Hess's, or Harold Stevenson's comparative work in Japan and the United States – reported in the *Academy Notes*, volume 13, number 2 – is not only giving us a clearer idea of the conditions of intellectual attainment in childhood, but producing a differentiated view of plasticity in performance that is directly relevant to educational practice.

**. . . mass schooling was the major force behind the fertility transition in the West, and continues to be elsewhere. As important as demographic transition is, it is only one example of the broad and unplanned change in attitudes and behavior brought about . . . by the expansion of schooling – a topic worthy of further research.**

The third task of educational research I mentioned at the outset is the description and explanation of intended and unintended consequences of educational policy. Here I return to the global perspective and comparative research. We are in the midst of an unprecedented revolution in human development associated with the onset, in the nineteenth century, of mass schooling in the West and Japan, and its spread to other parts of the world in our own time. The recency of this phenomenon can be illustrated by the fact that only in this century has a substantial proportion of any national population attended secondary school, though secondary schooling, now compulsory in many countries, had been widely available earlier. The global expansion of Western patterns of schooling and its extension in the lifespan have, of course, had negative as well as beneficial effects, but it is only recently that systematic research has shown just how powerful and predictable many of the effects are. Perhaps most surprising is that, whatever effect schooling has on the acquisition of academic skills, it seems to strongly affect other behaviors, not deliberately aimed at, but of great importance. The data analyzed by Susan Cochrane (1979) and her colleagues at the World Bank, for example, show the consistent association of reductions in fertility and infant mortality with individual educational attainment in Third World countries, suggesting a major shift in family life related not only to the expansion of schooling in general, but the extension of opportunities for formal education to women. This view is supported by John Caldwell's (1982) research and argument that mass schooling was the major force behind the fertility transition in the West, and continues to be elsewhere. As important as demographic transition is, it is only one example of the broad and unplanned change in attitudes and behavior brought about in our own and other societies by the expansion of schooling – a topic worthy of further research.

On the side of *intended* consequences of the global spread of schooling, I would like to mention the research of Steven Heyneman (1983) at the World Bank showing that, when the poorer countries are analyzed separately, the effects of school quality variables on examination performance are stronger than those of home variables, though the opposite is found in the wealthier countries. Painstaking comparative research can reveal the limits to generalizations based on educational research in our own society, with a net gain in

**Painstaking comparative research can reveal the limits to generalizations based on educational research in our own society, with a net gain in clarification on both sides, and the opening up of new fields for research related to educational policy.**

clarification on both sides, and the opening up of new fields for research related to educational policy.

The global schooling phenomenon, then, whether examined as an unplanned revolution in attitudes affecting many areas of life, or as a planned educational intervention that works out differently in different places, promises to teach us a great deal about the nature of social change and the nature of schooling.

These examples suggest that comparative and developmental research have helped redefine education and educability for us. We have begun the work of exploring the outcomes of the world's newfound commitment to schooling. Other examples could have served as well to illustrate that comparative and developmental research not only extends what we know, but transforms our view of what is possible in the field of education.

**References**

Bloom, B. In press. *Developing talent.*

Bloom, B. and L. A. Sosniak. 1981. Talent development vs. schooling. *Educational Leadership,* 39 (2).

Caldwell, J. C. 1982. *Theory of fertility decline.* New York: Academic Press.

Cochrane, S. H. 1979. *Education and fertility: What do we really know?* Baltimore: Johns Hopkins University Press.

Cole, M. and S. Scribner. 1973. Cognitive consequences of formal and informal education. *Science,* 182: 553-559.

Dweck, C. and E. Elliott. 1983. Achievement motivation. In P. Mussen and E. Hetherington (Eds.), *Carmichael's Manual of Child Psychology: Social and Personality Development.* New York: Wiley.

Heyneman, S. and W. Loxley. 1983. The impact of primary school quality on academic achievement across twenty-nine high and low income countries. *American Journal of Sociology,* 88(6):1162-1194.

Schaie, K. W. 1965. A general model for the study of developmental problems. *Psychological Bulletin,* 64:92-107.

Schaie, K. W., G. Labouvie-Vief, and B. U. Buech. 1973. Generational and cohort-specific differences in adult cognitive functioning: A fourteen-year study of independent samples. *Developmental Psychology,* 9:151-166.

Schaie, K. W., and G. Labouvie-Vief. 1974. Generational versus ontogenetic components of change in adult cognitive behavior: A fourteen-year cross-sequential study. *Developmental Psychology,* 10:305-320.

Scribner, S. and M. Cole. 1982. *The Psychology of Literacy.* Cambridge, Mass.: Harvard University Press.

Vogel, E. 1962. Entrance examinations and emotional disturbances in Japan's "New Middle Class." In R. Smith and R. Beardsley (Eds.), *Japanese Culture: Its Development and Characteristics.* Chicago: Aldine.

# Overeducation or Undereducation?

Jacob Mincer

*Jacob Mincer teaches courses and leads seminars in the economics of labor, education and population. His research centers on the relation between human capital formation, including education, training, and labor mobility, and the structure of wages and of employment. In the summer of 1983, Professor Mincer co-directed an international conference in Sussex, England on the causes and consequences of trends in women's labor force participation. He is currently Professor of Human Relations and Economics at Columbia University.*

Recent sessions of the American Economic Association devoted to education indicate a swing of the intellectual pendulum away from the not-so-long-ago fashionable revisionist notions, which ran from arguing the near-uselessness of education as an overly costly ability sorter to asserting its positively harmful role in the "great training robbery," or in the capitalist brainwashing conspiracy. The papers presented at the sessions also appear to be antidotes to the latest form of neoMalthusianism, which sees dire consequences in the demographic and educational expansion of recent cohorts.

It is interesting to note that academic views of education tend to be countercyclical: Complaints about sinister aspects of traditional education became fashionable at the height of the educational expansion in the late sixties; of late, praises are audible once again as financial retrenchment, a slack economy, and demographic declines are reducing the demand for education. Perhaps this is mere contrariness, but to the extent that economists' voices are heard, they may perforce be carrying out their accustomed conservative function of mitigating the succession of wide swings in our social climate.

What follows is a review of and response to recent analyses of the economics of education by economists, revolving around the question of whether our school systems are producing over or undereducation.

**Education and Economic Growth**

**. . . to the extent that economists' voices are heard, they may perforce be carrying out their accustomed conservative function of mitigating the succession of wide swings in our social climate.**

The interest in the economics of education originated simultaneously and independently in two fields: in the study of its role in economic growth, and in the study of its effects on the wage structure in the labor market. It soon became apparent that the concept and analysis of investment in human capital, which is more general than school education, is common to both. Economic growth is, indeed, the focus in both applications: Just as accumulation of personal human capital is a factor in individual economic growth, so it is at the aggregate (national, and even international) level. Although there has been no shortage of warnings against a possible fallacy of composition, the proposition remains theoretically and empirically viable in the large.

Educational activities, broadly construed, involve not only the transmission and embodiment in people of available knowledge, but also the production of new knowledge, which is the source of innovation and of technological change. The new knowledge and technical progress are diffused throughout industry and across national boundaries, creating national and worldwide economic growth. If the rates of technical change and of its diffusion are complementary with education, the marginal product of education is augmented by the rate of technical progress. At the aggregate level, therefore,

**. . . education is a factor of production that is both a cause and a consequence of economic growth. Unquestionably, such feedbacks play a large role in the long-term dynamics of the economy.**

education is a factor of production that is both a cause and a consequence of economic growth. Unquestionably, such feedbacks play a large role in the long-term dynamics of the economy.

Estimates of effects of education on economic growth are understated if they confine themselves to effects on the quality of labor, and leave out the effects on technological change. It follows that the usual estimates of rates of return to education may be understated, if effects on the growth of technology are not captured in the private returns of those contributing to it.

The extent to which such contributions are captured in private returns, of course, varies. Still, the 75 percent decline in the (relative) production of engineers and the 50 percent decline in the production of scientists (at the B.S. level) in the United States, over the past twenty years, as well as the decline in the volume of private and public R&D, are disquieting, whether or not the decline is a result or a partial cause of slow economic growth.

**Overeducation?**

**The decline in productivity and in economic growth in the 1970s set in just about the time when large numbers of young college graduates began to enter the labor market . . . .**

The decline in productivity and in economic growth in the 1970s set in just about the time when large numbers of young college graduates began to enter the labor market. The increased supply of college (and college plus) graduates in a deteriorating labor market were bound to experience at least temporary difficulties in job finding and in the quality of jobs and careers they found and settled for.

Richard Freeman dubbed the atypically large cohort of college graduates the "Overeducated Americans." His evidence of "overeducation" was the decline in relative incomes of college graduates compared to high school graduates in the 1970s. As James Smith and Finis Welch pointed out in the 1978 *Proceedings* of the National Academy of Education, this decline was overstated (doubled) by Freeman's procedure. Moreover, the large cohort burdens of an increased labor supply, while aggregate demand was falling, was shared by all education groups and not merely by those at the college levels, though wage effects (reductions in relative wages) and some initial slowing of progress were somewhat greater at the college level than below. Smith and Welch believe this setback to be temporary. A 10 to 15 percent reduction in starting incomes, *relative* (not absolute) to incomes of older workers in the same education cohorts hardly justifies long-term dire consequences predicted by Richard Easterlin (1978) or the more cosmic ones of Landon Jones (1980).

Equating "overeducation" with a rapid increase in labor supply, due to the past baby boom, is a hyperbole: Oversupply is equivalent to "underdemand," and as supply and demand fluctuate there will always be an adjustment period during which there are shortages and surpluses, and new graduates may

be called "overeducated" or "undereducated." But, even when defined solely in economic terms, what matters is the rate of return on the educational investment, and although this declined somewhat in the seventies, the rate is not so small that the volume of graduates turned out by schools should be deemed excessive.

**. . . rates of return restricted to monetary payoffs understate the true rate: They omit some of the labor market returns in the form of fringes and of non-pecuniary job benefits, including congenial working conditions, and they exclude a variety of non-market benefits.**

Indeed, there are at least two reasons to believe that the calculations of rates of return restricted to monetary payoffs understate the true rate: They omit some of the labor market returns in the form of fringes and of nonpecuniary job benefits, including congenial working conditions, and they exclude a variety of nonmarket benefits.

The argument is not new. It was expressed by Theodore Schultz many years ago, but the list of omissions is now more solidly informed by the intervening research in human capital and in household production (or in the so-called "new home economics").

Among the benefits overlooked are: (1) fringe benefits – these are proportionally larger for the more educated, whose higher marginal tax rates, due to higher incomes, induce a demand for (proportionately, not just absolutely) larger fringes; (2) apparent increases in productivity in other forms of human capital formation, such as job training, household management, and health maintenance (I say "apparent" since, in part, the relation is a correlation due to common factors, rather than cause and effect.); (3) better sorting in job and marriage markets, reduction in crime, informed migration and citizenship, which are the other more or less documented benefits.

In a paper at the 1982 AEA session, Robert Haveman and Barbara Wolfe attempted to convert to monetary values the nonwage and nonmarket contributions of education in order to correct the usual narrowly defined estimates of returns to education. Their guesstimate is that complete returns would probably double the usual numbers. This result, they conclude, suggests that the "overeducated American" is a myth, despite the declines in the narrowly calculated rates of return in the mid 1970s.

### Or Undereducation?

At this point, it is helpful to reflect about the concept of education in contrast to schooling, since the discussion thus far refers to schooling, that is in terms of educational attainment measured exclusively by number of years spent in school. The term "overeducated" is unfortunate for more reasons than indicated, although the terms "overschooled" or "mis-schooled" are not necessarily inappropriate. If we think of schooling as an input, and education, in its basic cognitive sense, as the output, perhaps what we experienced in the late 1960s and early 1970s was an expansion or even overexpansion of schooling, but no improvement, and perhaps a deterioration, in education. Such a decline in the *productivity* of schooling *pari passu* with the much

**If we think of schooling as an input, and education, in its basic cognitive sense, as the output, perhaps what we experienced in the late 1960s and early 1970s was an expansion or even over-expansion of schooling, but no improvement, and perhaps a deterioration, in education.**

discussed decline in productivity in the market economy, may be a coincidence or it may reflect some common causes, even if there were no direct connection between them. Still, both may be responsible in some measure for the observed declines in the calculated rates of return to schooling, apart from the much (and I think too much) touted invasion of the labor market by the baby crop of the 1950s armed with diplomas.

Suggested evidence on the performance of our school system comes from cross-national comparisons and changes over time in scholastic achievement. Twelve volumes of studies published between 1973 and 1979 by the International Association for the Evaluation of Educational Achievement describe results of (identical) achievement tests in eight subjects given to 10-, 14-, and 18-year-old students in twenty-one countries.

The National Academy of Education did review the IEA Volumes in 1977. In his review in that year's *Proceedings,* Alex Inkeles made a number of inferences, such as: (1) "The more emphasis on a subject in the curriculum and more time students spend on it, the more knowledge and skill they show in the subject." (2) At least 80 percent of the variance in test scores remain to be explained by factors other than socio-economic background. (3) School quality alone accounts for as much as home background, age, and sex taken together. Inkeles concluded that despite the great value of the endeavor, the results have been left seriously underanalyzed, for reasons he enumerated, but which are not insuperable. Inkeles did not indicate the relative standing of American students in the international comparisons.

In a more recent analysis of the IEA volumes, Barbara Lerner finds that the mean scores of American high school seniors were in the bottom half of the rank order distribution thirteen times and in the top half only six times, when four LDCs were excluded. Results for younger students (14 and 10 years of age) were somewhat better, but not much. American students ranked especially low in mathematics and science. Yet the volume of inputs, as measured by educational expenditures per capita or fraction of GNP spent on education were (among the) highest in the United States.

Although Inkeles tends to agree with some of the IEA authors (especially in Volume 9 of the series) that larger retention rates (larger proportions of the age group enrolled) result in lower scores at the 18-year-old level, Miss Lerner claims that these differences carry little weight, especially as the rankings are similar at the younger ages (10 and 14), when retention rates do not differ much among the developed countries.

Although these findings refer to the late sixties, changes since then have not been salutary. The observed declines in SAT and other achievement tests date from that time, and again cannot be blamed on increasing high school retention rates, which were stabilized by 1970. Similar declines have been observed in the upper grades of elementary schools.

**If spending and retention rates do not explain the cross-national differences and the time-series changes, what does?**

If spending and retention rates do not explain the cross-national differences and the time-series changes, what does? Miss Lerner finds four variables to be significant: amount of time spent on homework, amount of class time spent directly on relevant school work, frequency of class attendance, and textbook demand levels. Of those, the first, which she calls "hard work," appears to dominate. She also cites evidence of decline in all these factors in American schools.

Declines in test scores for grades 5 through 12 have been analyzed by several researchers, as reported in the NAE volume *Education for Employment*, 1979. For instance, Wiley and Harnischfeger (1977) analyzed a number of test scores and their changes over time. They concluded that "basic reading skills are improving, but the more advanced skills of comprehension and interpretation are on the wane." Of a variety of factors considered, the analysts were inclined to think most important "changes in the amount of time spent studying and modifications in the curriculum." They noted students taking fewer courses in traditional academic subjects such as history, foreign languages, algebra, geometry, chemistry, and physics. Fine reported in 1966 declining class time devoted to instruction and increasingly devoted to administrative and disciplinary activities. Others have noted the relative decline in scores of students who apply to teachers' colleges. On the whole, these findings are consistent with Lerner's blunter statements.

**Perhaps fixed ages of school entrance and completion are as much out of tune with changes in personal development and health as fixed ages of retirement.**

The high and perhaps even rising levels of absenteeism, reported in the popular press especially for upper high school grades, may be both a cause of and a response to the observed problems. Perhaps fixed ages of school entrance and completion are as much out of tune with changes in personal development and health as fixed ages of retirement.

The effects of changes in the quality of education on measured rates of return to schooling are matters of concern to econometricians. But the problem of low quality of education is of urgent concern to our society and economy.

**References**

Easterlin, R. 1978. What will 1984 be like? Socio-economic implications of recent twists in age structure. *Demography.* 15 (4), 397-432.

Freeman, R. 1976. *The overeducated American.* New York: Academic Press.

Inkeles, A. 1977. A review of *International studies in evaluation* (9 volumes). *Proceedings of the National Academy of Education, 4,* 139-200.

Jones, L. 1980. *Great expectations: America and the baby boom generation.* New York: Coward & McCann.

Lerner, B. 1982. American education: How are we doing? *Public Interest, 69,* 59-82.

Smith, J. and F. Welch. 1978. The overeducated American? *Proceedings of the National Academy of Education, 5,* 49-83.

Task Force on Education and Employment. 1979. *Education for employment: Knowledge for action.* National Academy of Education.

# Cognitive Science as Educational Research: Why We Need It Now

Lauren B. Resnick

*Lauren B. Resnick is Professor of Psychology and Education and Director of the Learning Research and Development Center at the University of Pittsburgh. Professor Resnick's primary interest is the emerging field of the cognitive psychology of instruction. Her current research focuses on the learning of mathematics and science. She has studied reading and other subject-matter domains as well, and is founder and editor of* Cognition and Instruction, *a new journal in the field. Professor Resnick serves on a number of national and international boards and commissions, and is active as a consultant to foundations and government agencies. She is past president of the APA's Division of Educational Psychology, and a fellow in the Association's divisions of Experimental Psychology, Developmental Psychology, Educational Psychology, and the Experimental Analysis of Behavior. She also has served as Vice President of the American Educational Research Association's Division of Learning and Instruction, and is a fellow of the American Association for the Advancement of Science.*

**Although much public rhetoric focuses on declining achievement and on the hope for a return to the standards of the past, today's goals cannot be met with the methods of the past. We must now make it possible for everyone to meet standards of educational performance that once were expected of a much smaller segment of the population.**

Our nation has a long history of commitment to the goal of building an educated citizenry. With each decade, we have increased the proportion of the population in school and extended both the range of social groups of concern to the school and the amount and kinds of education offered. Our goal is half met: Virtually all children today attend school. Now we must turn our full attention to the quality of the education they receive.

The present challenge is to teach successfully all of the diverse children who have become the active concern of our educational system. Although much public rhetoric focuses on declining achievement and on the hope for a return to the standards of the past, today's goals cannot be met with the methods of the past. We must now make it possible for everyone to meet standards of educational performance that once were expected of a much smaller segment of the population. We want to raise average performance without sacrificing our traditional standards of excellence. This goal poses a complex new task for educators. In the not-so-distant past, many problems of education were solved by selection: If individuals did not learn easily and study with enthusiasm, it was acceptable to drop them from the educational effort. Now that we have made a national commitment to raising levels of educational performance for all children, dropping the reluctant or difficult learner is no longer an acceptable solution. Instead, we must find ways to teach even the hardest to teach, to inspire even the most reluctant.

This new challenge demands sustained research on processes of learning and teaching. We have inherited theories and practices that are inadequate to the educational tasks we now face. Developed in an era when selection more than instruction was the organizing principle for educational success, our theories of instruction and our schooling practices do not meet the demands today's schools confront. Although we could tolerate a relatively weak science of education, as long as we sought to teach only the willing and particularly able, we need a stronger theoretical base if we are to serve a far broader range of students. This essay examines the potential contributions of the emerging discipline of cognitive science to the construction of the educational theory needed to craft educational practices responsive to this new challenge.

Cognitive science can best be viewed as a loose federation of older disciplines – psychology, linguistics, computer science – all concerned with aspects of human mental functioning. In offering a powerful reconceptualization of the nature of the learning process and new methods for the investigation of learning, it can be invaluable to education. Complex methods for inferring mental processes from empirical data now exist and are being applied increasingly in analyzing problems of school learning. At the same time, the growth of information-processing theory offers an important new tool for theory-building in the domain of complex intellectual processes. Today, the combination of methods available for research on cognitive processes and the constructs that are among the intellectual tools of cognitive

science make possible the systematic study of critical educational questions. We have deepening scientific understanding of performance on complex reasoning tasks, the processes by which skill and understanding in mathematics, science, and other disciplines are learned, the influence of people's existing knowledge on how they learn new information and concepts, the processes involved in understanding and writing texts of various kinds, and the nature of aptitudes and skills for learning.

**The general shape of an educationally relevant cognitive science has become apparent. It can be expected to pay off handsomely in a reasonable period of time because it confronts directly questions to which educators need answers . . . .**

Although it is not yet ready for prescriptive application, cognitive science is poised for progress in research that promises a truly solid basis for revisions of educational content and practice. The general shape of an educationally relevant cognitive science has become apparent. It can be expected to pay off handsomely in a reasonable period of time because it confronts directly questions to which educators need answers:

*(1) What are the mental processes typical of experts in reading, writing, mathematics, problem solving, etc?* Research on this topic will allow us to define teaching goals that focus clearly on mental capabilities.

*(2) How do people acquire the processing capabilities of experts?* Research on acquisition will help to establish what learning processes instruction should try to activate and to suggest what kinds of specific knowledge may be needed before learners can benefit from particular kinds of instruction.

*(3) What are the effects on mental processes of different kinds of instruction?* Inquiry in this area will provide a scientific basis for the improvement of instruction because it will tell us not just whether an instructional program succeeds, but why.

These three questions provide a framework within which fruitful cognitive research on education can proceed. I can best give a sense of the possibilities by describing briefly research now being pursued in three areas of educational concern.

**Reading and Writing**

Recent research on reading comprehension has produced enormous changes in our conception of reading. We now think of the reader as a constructor of meaning rather than a recorder of messages, and we recognize that a text provides only part of the information that readers need to make sense of the situation which it describes. Teaching practice is at odds in several fundamental ways with this new conception of the reading process. First, current instruction does not explicitly convey to learners the centrality of their own role in the process of understanding. As a result, children are taught neither to monitor their understanding nor to apply consciously strategies for correcting misunderstandings or incomplete understanding. Second, most reading programs do not systematically build on the topical

knowledge that students already have. Third, little attention is given to helping students build up the knowledge of basic forms of discourse that could guide their attempts to interpret texts of various types. Finally, school texts are constructed with little or no attention to the new cognitive principles that explain what makes texts easy or difficult to understand. Thus, they are weak vehicles for either teaching specific content or sharpening general reading skill.

**What is now needed, if we are to profit educationally from the changed conception of the reading process, is research that can not only criticize current teaching practices but also suggest how to reform them.**

What is now needed, if we are to profit educationally from the changed conception of the reading process, is research that can not only criticize current teaching practices but also suggest how to reform them. Such work is growing at the present time. Research is also growing on the question of how skill in the processes of interpreting texts is *acquired*. At the same time, we are beginning to see experiments that explicitly examine the ways instruction can modify reading processes. The following are among the issues explored in this instructional experimentation:

We know that *prior knowledge of the subject matter of a text* is important in interpreting it, but we do not yet know how to use this fact in teaching reading. Some current research is exploring the effects of restructuring the oral discussion that accompanies reading stories in instruction. A more radical approach would be to build reading instruction around topical areas of study. Both approaches to helping children use prior knowledge in reading raise the question of how, and indeed whether, generalized reading skills develop when instruction and practice center on a particular domain of knowledge or on the full understanding of individual stories. This is a fundamental cognitive question that can probably only be studied in the context of instructional experiments designed to permit detailed analysis of reading processes.

A related line of experimentation concerns *teaching vocabulary and vocabulary's place in general comprehension skill*. We know that large vocabularies and good reading skill are associated, but efforts to teach vocabulary directly have not usually bolstered general reading skill. Recent research has shown that new methods of teaching vocabulary – ones that accord well with cognitive theories about how word meanings are stored in memory and retrieved – can improve reading comprehension markedly.

Another productive line of instructional experimentation is concerned with *teaching children consciously to monitor and control their reading processes*. Some very recent studies give us reason to be cautiously optimistic about the instructional possibilities in this area. Nevertheless, more small-scale experiments, accompanied by detailed analyses of the cognitive effects, are required before we could responsibly advocate this kind of teaching as a regular part of the school program.

With regard to composition, there can be little doubt that writing skills are not well developed by America's schools. The main problem, at the moment,

**Given the virtual absence of serious writing in American schools, even weak programs are likely to produce improvements. We are unlikely, however, to foster proficiency in all the new populations now in school with programs that are not well grounded in knowledge of the nature of writing skill and how it is acquired.**

is less that composition is taught badly than that it is hardly taught at all, and that very little writing of connected discourse is required over the course of the school career. Given the virtual absence of serious writing in American schools, even weak programs are likely to produce improvements. We are unlikely, however, to foster proficiency in all the new populations now in school with programs that are not well grounded in knowledge of the nature of writing skill and how it is acquired.

To develop more powerful forms of instruction in writing we need a more precise body of knowledge and theory. Although research on composition is not as well developed as research on reading, an important research capability has grown up in the last few years – in large part a response to public concern about writing skill. People from several scholarly disciplines – psychology, linguistics, and English – are conducting cognitive research on composition and ways of teaching it. There is thus a solid basis for progress in the cognitive analysis of writing skill, and much of the research is directly concerned with instruction.

### Mathematics, Science, and Problem Solving

**. . . improved specifications from cognitive analyses of what we mean by "understanding" mathematics, can provide a basis for an eventual reform of the curriculum that can go beyond sterile debates over "new" versus "old" math, or "process-" versus "fact-" oriented science teaching.**

As in the case of reading, the new cognitive research is productively reformulating some of the old divisive questions, such as whether elementary mathematics teaching should aim at understanding basic concepts or at skill in calculation and whether the emphasis of school science should be on processes of problem solving or on the established facts and principles of each field. Instead, cognitive scientists are now raising such questions as: (a) how computational skill and understanding interact, (b) how understanding may develop as a result of practice, (c) what the role of language and explanation in the development of mathematical and scientific thinking is, and (d) how scientific ideas can be presented to make students' almost inevitable inventions more likely to be correct rather than errorful. Answers to these questions, along with the improved specifications from cognitive analyses of what we mean by "understanding" mathematics, can provide a basis for an eventual reform of the curriculum that can go beyond sterile debates over "new" versus "old" math, or "process-" versus "fact-" oriented science teaching.

What we know about learning and problem solving in math and science comes from a relatively small number of studies that share an important characteristic: They have focused on the analysis of a particular topic in the curriculum. There is strong reason to believe that this strategy of "cognitive curriculum analysis" will continue to be fruitful, both as a way of providing clearer objectives for instruction and as a way of revealing processes of learning and instructional methods capable of positively channeling those processes.

### General Skills for Learning

**Up to now, researchers have not been highly successful in finding teachable skills that generalize broadly. Where there have been demonstrable successes, the skills taught have usually been specific to some subject matter....**

Cognitive research is also providing a fresh outlook on the question of the potential role of schools, or other educational institutions, in improving children's learning skills. Recent research has uncovered significant similarities in patterns of problem solving and learning in different domains of thinking, and this has produced continued optimism about the possibilities for teaching certain general skills of learning and problem solving. The task ahead is to discover how to do this and to what extent the optimism is justified. Up to now, researchers have not been highly successful in finding teachable skills that generalize broadly. Where there have been demonstrable successes, the skills taught have usually been specific to some subject matter, for example, how to analyze and solve math problems, how to study physics, or how to decide you do not understand something as you read.

A promising recent development has been a shift in focus from these specific strategies to more general abilities to monitor one's own learning and then decide *when* to apply specific strategies. These general monitoring abilities are part of what cognitive scientists call "metacognition" – knowledge of one's own cognitive processes. Several studies have established that metacognitive knowledge develops gradually and that it is weak or lacking in some of our poorest learners. A small number of recent studies have focused on teaching people to become aware of their processes of thinking and to assess their own current state of knowledge. There are some modest successes to report from studies in which individuals have shown improved learning over fairly extended periods of time and in settings not identical to those in which they were trained. There is clearly reason to continue this promising research by both expanding the range of specific learning skills taught and trying to press the limits of their generalizability and applicability. Instructional experiments of various kinds are needed – not only to establish "what works," but also to show how thinking and learning processes are actually modified and which aspects of instruction are most important in inducing these changes.

### The Time Lines for Applying Educational Research

The above are but a few examples of cognitive research in education. I have tried here only to suggest some lines of research that can contribute to a revitalized theory of teaching, one based on more precise knowledge of the processes of thinking and learning than we have had in the past. But when will it be reasonable to expect practical results for education?

I am in general accord with several authors in this collection who argue that research is absorbed into educational practice gradually and that effects of research on educational practice are generally easier to detect retrospectively than they are to predict or plan. Nevertheless, I think it is possible to focus research efforts – even in fundamental domains such as cognitive science –

in ways that maximize the probability that educational implications will be drawn and used.

The strategy I propose is one that centers on research likely to provide grounds for effective new practices in the middle term – within 10 to 15 years. At present there are strong reasons to focus on middle-term goals rather than on short-range efforts to apply what we know. The short-range strategy, attractive because it seems to promise a quick result with obvious social benefits, is only likely to work when there exists a well-developed body of knowledge suitable for application. At other times, the pressure to bring about immediate improvements will result either in repeating past innovations, with but slight variations, or in pressing toward large-scale applications of not yet adequately established ideas. A strategy for the middle term avoids these pitfalls by asking which aspects of the basic science are already poised for research along lines likely to benefit educational theory and practice, and then seeking to promote activity in those areas.

**We need to build on the extant research base in cognitive science, to refine the general principles by focusing on learning in domains that are central to the school curriculum and to fill out the emerging picture of learning in various subjects.**

This is, I believe, a good time for middle-term investment in cognitive research on learning and teaching. As several authors in this collection have shown, we have learned much from past efforts to apply associationist, behavioral, and traditional developmental theories to education; and many of these principles have been absorbed into mainstream educational practice. These older theories, however, are no longer producing truly fresh ideas for education. By contrast, cognitive science is capable of genuinely refocusing and re-energizing important aspects of educational theory and practice. It is not yet ready, however, to make tight prescriptions for educational reforms. We cannot say yet that we already know what will work and only need to find ways of making these things happen in schools. Our current cognitive principles are too broad and our specific findings often too fragmentary to be directly applied. We need to build on the extant research base in cognitive science, to refine the general principles by focusing on learning in domains that are central to the school curriculum and to fill out the emerging picture of learning in various subjects. A robust science of cognition that can directly benefit education will emerge from these efforts.

# Understanding Pedagogy: Research for the Improvement of Teaching and Teacher Education

Lee S. Shulman

*Lee S. Shulman is Professor of Education and Affiliated Professor of Psychology at Stanford University. His research centers on the activities of two professions: teaching and medicine. Shulman is interested in the cognitive psychology of practice in those two fields, in how both teachers and physicians make judgments and decisions, make sense of the world around them generally, and learn to act professionally in the face of problems. The formal and informal methods he designs to prepare individuals professionally – programs of teacher education and medical education – must be studied and experimental programs to improve such preparation pursued. Professor Shulman also has longstanding research interest in the logic of educational research methodology, the psychology of curriculum and instruction (especially in science, mathematics and medicine) and the effects of educational policy on teaching.*

Over the years, I have been continually frustrated in my desire to invent a particularly cogent idea or to craft a memorable *bon mot*. Whenever my hopes rise, I discover that someone else had stumbled across – and published – the same notion. My particular nemesis over the years has been John Dewey, whose capacity for "anticipatory plagiary," in Robert Merton's eternally apt phrase, strikes me as boundless. Shortly after sketching out the few ideas that comprise this brief essay, I chanced again to read Dewey's 1904 NSSE Yearbook chapter, "The Relation of Theory to Practice in Education." In that paper, Dewey examines the role of practical experience in the preparation of teachers. In his excursion, he also manages to say nearly everything sensible there is to say about the relationships between the development of educational theory and the improvement, through teacher education, of educational practice. Although not every reader – not even I – agrees with all the particulars of his argument, we cannot ignore the prescient quality of his observations. He maintained a deeply held belief in the improvement of practice through the application of systematic inquiry, yet he avoided seduction by naive scientism. He recognized that practice and the wisdom acquired only through reflective experience are indispensable in preparing excellent pedagogues. And he firmly asserted that continued educational research, which would produce better educational theory, would contribute to the improvement of educational practice. Nearly eighty years later, we find ourselves examining the warrant for this Deweyan optimism.

I take, as my charge here, discussion of the study of teaching and learning, (especially the former) and its utility for the improvement of pedagogical practice through teacher preparation. Few areas of contemporary educational research are as active and fruitful as the study of teaching. The teacher, the classroom, and the total school organization are being systematically scrutinized for the secrets of effectiveness. Correlational and experimental studies in abundance address such questions as "Why are some teachers more consistently capable of raising students' achievement test scores?" "What approaches to classroom management raise student performance?" "What characterizes the schools whose students excel during their tenure within them?"

**. . . the current genre of research on teaching and learning requires little cross-context extrapolation. Real teachers and real students participate; the topics are two-digit subtraction and group performance in comprehending paragraphs or proving geometric theorems; and the settings are typical classrooms . . .**

Moreover, the classroom and school have deepening intrinsic interest for social scientists, who find those settings as fascinating as more exotic environs were to an earlier generation of investigators. Although these scholars' efforts may be less self-consciously directed at the improvement of practice than were effectiveness researchers', their studies are likely to yield equally impressive guidance. Unlike an earlier epoch, when research conducted in the experimentalist's or psychometrician's laboratory was arduously extrapolated to the pupil and the classroom, the current genre of research on teaching and learning requires little cross-context extrapolation. Real teachers and real students participate; the topics are two-digit subtraction and group performance in comprehending paragraphs or proving geometric theorems; and the settings are typical classrooms in conventional schools.

In the balance of this paper, I will review several of the areas where research on teaching and learning has been particularly active and successful. I will then discuss how these findings (and others like them) become "useful" for the improvement of educational practice, especially through teacher preparation.

One of the most frequent outcomes from studies of teaching, especially those conducted in the "process-product" tradition, are propositions describing the relationships between teacher behavior and student performance in particular contexts. Studies like these yielded many of the compelling findings about time-on-task (and its more sophisticated cousin, academic learning time); patterns of turn-taking in small groups (and the surprising superiority of ordered turns for achievement in first-grade reading groups); the general features of the direct instruction and active teaching models associated with the work of Nathaniel Gage, Susan Brophy, Thomas Good, David Berliner, Barak Rosenshine, Jane Stallings and others; and approaches to classroom management and control such as presented by Jacob Kounin or Carolyn Evertson. The propositions rarely achieve the level of integration or coordination expected of more elaborated theories of instruction. They are empirical without embarrassment, each proposition standing as a principle of practice. In field studies or in development of teaching manuals, they are aggregated into lists of suggestions or programs of practice. But there is rarely an attempt to subsume whole sets of propositions into a unified hypothetico-deductive model.

The propositions stemming from this kind of research are convincing and useful to practitioners and policymakers. Practicing teachers find the admonitions intuitively sensible and frequently helpful. Advocates of such research point out that its findings always grow out of observations of what some practitioners are already doing in classrooms. They point out also that, in principle, the recommended behaviors and practices must therefore be possible. (Although this may be the case for individual findings, nothing in the correlational data makes it necessary to believe that particular combinations of behavior were likely to appear together naturally for an individual practitioner.)

A second type of research has yielded increasingly rich descriptions of the complexities of social interaction in classrooms and schools. These studies bring alive the day-to-day realities of life in classrooms, conveying the prosaic regularities of the typical in some studies and the provocative and unusual innovations driving radical change in others. They complement the cool, decontextualized, and disaggregated generalizations of process-product research with the specific and local particularizations of the case study or portrait. They frequently serve as the exemplars or cases-in-point necessary to bring principles to life, or, on the other hand, to forestall temptation to treat principles as universal mandates for practice.

A third type of classroom research shifts the focus of inquiry from the teacher to the taught, and from the processes of teaching to the processes of attention (or inattention), apprehension (or misapprehension), and general cognitive functioning in pupils during and after instruction. This research asks what is going on in the heads of learners as a result of variations in teaching that the researchers observe in the classrooms. These characterizations are couched in theoretical terms like those used by cognitive psychologists to explain student learning the subject areas. In this sense, the research on student mediation of teaching, on the ways pupils actively extract information and make sense of the pedagogical whirl of their classrooms, serves as the bridge between research on teaching and research on learning. In a theoretically significant fashion, many of the processes of both teaching and learning are now under investigation using a common theoretical language, that of cognitive psychology and its social science cousins.

**. . . we can envisage the development of curriculum-relevant, developmentally organized casebooks or Merck Manuals, in which the predictable pathologies of school learning can be listed and their indicators discussed.**

As other papers in this collection report, the psychology of school subjects is experiencing a renaissance reminiscent of the heyday of E. L. Thorndike and Charles Judd. Especially in mathematics, physics, reading comprehension, written composition, and study (metacognitive) skills, far more precise accounts can now be rendered of the acquisition, transformation and use of subject-matter specific knowledge. Detailed understanding of what makes learning (and hence teaching) difficult in these areas is emerging. As the studies accumulate results, we recognize the naive theories that learners bring to bear in acquiring new knowledge of, for example, the physical world and how such theories encumber their abilities to acquire new understandings. We also are beginning to understand the most typical misconceptions associated with particular topics at particular ages. Unlike the Piagetian work of the last generation, however, the tasks studied are based on typical school-based curriculum topics and not on ostensibly universal, school-independent accomplishments like the concept of conservation of liquid volume. As such work progresses, we can envisage the development of curriculum-relevant, developmentally organized casebooks or Merck Manuals, in which the predictable pathologies of school learning can be listed and their indicators discussed. Furthermore, the most successfully employed remediations for each type of misconception can be presented and explained. Such elaborated systems of pedago-pathology will be valuable tools in the teacher's kit, though they will not, in themselves, dictate teaching methods.

**Learning to teach is certainly no less complex than learning physics, geometry, and other school subjects that have commanded the attention of cognitive psychologists.**

We must avoid the temptation, while viewing student learning in increasingly complex and sophisticated ways, to treat teacher preparation – the learning of teachers – in a naive and simple-minded manner. Learning to teach is certainly no less complex than learning physics, geometry, and other school subjects that have commanded the attention of cognitive psychologists. The dramatic increase in our understanding of the complexities of subject-matter learning, the richness and variety of classroom and school social contexts, and the growing body of propositional knowledge regarding

relationships between teaching activity and pupil test performance all result in a far more extensive body of knowledge required for responsible teaching performance. Such principles must be assimilated into teachers' cognitive processes – their thinking, decision-making actions – if they are to gain regular use. As this conception of the knowledge base for teaching grows, our sense of what teachers must know and know how to do has grown more elaborated and ambitious, as well as become better grounded in research.

The study of teaching will, predictably, continue to grow, yielding a larger number of simple, theoretically powerful individual propositions. Such theoretical simplification, however, does not necessarily produce practical simplicity; general principles do not drive general policies. Correlational and experimental research on teaching produces principles that are simple, clarifying, and guiding, but which are fundamentally incompatible with other compelling, equally correct, and well-grounded principles. The possibility of making more simple, general, theoretical sense does not lead to the simplification of practice. Indeed, as our general knowledge grows with the addition of new propositions about teaching and learning, the complexity of those processes themselves will inevitably grow in the classroom context, where the simple propositions intersect.

**. . . as knowledge of research deepens, the number and complexity of the teacher's set of usable premises increases. Hence, teacher reasoning grows better grounded, but simultaneously more complex in its alternative paths.**

This observation regarding the relationship between research and practice is hardly new. Joseph Schwab's conception of theory in his analyses of "the practical" follows precisely this line. To the extent that theory clarifies and grows more powerful, it also becomes narrow, as a laser-like beam that brilliantly illumines a tiny area. John Dewey's analysis of teaching practice portrays theory in a similar fashion. In his critical appraisal of the research literature on teaching effectiveness, Gary Fenstermacher argued that the results of such research should not be taken as prescriptions for practice. Instead, they should become – as Thomas Green says of teaching competence – part of the premises of the practical argument in the mind of the teacher. Which is to say as knowledge of research deepens, the number and complexity of the teacher's set of usable premises increases. Hence, teacher reasoning grows better grounded, but simultaneously more complex in its alternative paths. For the chess grandmaster, knowledge of tens of thousands of game patterns does not yield simplicity, even when they are ordered hierarchically and economically in memory. But such memory certainly produces power, as its equivalent in pedagogical knowledge does for the capable teacher.

To summarize, research on teaching and learning is flourishing. The knowledge base of teaching is becoming richer in its empirically grounded general propositions and is slowly growing in the power of its theoretical formulations. Instructional systems or protocols, such as DISTAR, IPI, Mastery Learning, Active Mathematics Teaching, and the like, are adding further to the pedagogue's arsenal of research-based alternatives. The result is a formidable view of practice. As the knowledge of teaching grows, the

practical work of teachers increases in its intricacy and in its intellectual (and moral) demands. The responsibility of the individual teacher to sift, weigh, judge, combine, sequence, and test, in context, possibilities from this growing set of frequently incompatible alternatives grows heavier. The qualifications for becoming a teacher, the length and rigor of the preparation period, the need for periods of internship, supervision, and continuing professional development broaden proportionally. The utility of educational research will be witnessed, therefore, not in its unmediated influence upon classroom practice, through teacher-proof technologies or procedural protocols, but through the increasingly central roles played by well-educated teaching professionals.

# National Academy of Education

**President**
Robert Glaser

**Vice President**
Richard C. Atkinson

**Secretary-Treasurer**
Thomas F. Green

**Members**
Richard C. Anderson
Gregory Anrig
Bernard Bailyn
Gary S. Becker
Charles Bidwell
John Brademas
Urie Bronfenbrenner
Jerome S. Bruner
John B. Carroll
Jeanne S. Chall
Burton R. Clark
Kenneth B. Clark
Michael Cole
James S. Coleman
Lambros Comitas
Lawrence A. Cremin
Lee J. Cronbach
K. Patricia Cross
Mihaly Csikszentmihalyi
Nathaniel L. Gage
Robert M. Gagné
Nathan Glazer
John I. Goodlad
Edmund W. Gordon
Patricia A. Graham
Hanna H. Gray
Maxine Greene
James G. Greeno
Elizabeth P. Hagen
Oscar Handlin
Alexander Heard
Theodore M. Hesburgh
Roger W. Heyns
Harold Howe II
Philip W. Jackson
H. Thomas James
Carl Kaestle
Francis Keppel
Michael Kirst
Wallace E. Lambert
Hope Jensen Leichter
Robert A. LeVine
Seymour M. Lipset
James G. March
Wilbert J. McKeachie
John W. Meyer
Martin Meyerson
Jacob Mincer
Ewald B. Nyquist
Frederick Olafson
Paul E. Peterson
Diane Ravitch
Lauren B. Resnick
Frederick Rudolph
Israel Scheffler
Lee S. Shulman
Julian C. Stanley
Patrick Suppes
Martin Trow
David Tyack
Finis Welch
Clifton R. Wharton, Jr.

**Members Emeriti**
Bruno Bettelheim
Benjamin S. Bloom
Howard R. Bowen
Guy T. Buswell
Roald F. Campbell
Francis S. Chase
John L. Childs
Erik H. Erikson
John H. Fischer
John C. Flanagan
William K. Frankena
John W. Gardner
J. W. Getzels
Eleanor J. Gibson
Nelson Goodman
Robert J. Havighurst
Ernest R. Hilgard
Sidney Hook
Cyril O. Houle
Clark Kerr
T. R. McConnell
Robert K. Merton
James A. Perkins
David Riesman
Wilbur Schramm
Theodore W. Schultz
Joseph J. Schwab
B. Othanel Smith
Robert L. Thorndike
Ralph W. Tyler
Willard Wirtz

**Foreign Associates**
Hiroshi Azuma
Joseph Ben-David
Michel Crozier
A. H. Halsey
John F. C. Harrison
Torsten Husén
Michio Nagai
Harold Noah
Richard S. Peters
Nathan Rotenstreich

**Foreign Associates Emeriti**
Eric Ashby
C. E. Beeby
Barbel Inhelder
Jan Szczepanski
Philip E. Vernon

**Executive Director**
Michelle von Koch

Michelle von Koch edited and directed the production of this report; Frank Garrity Graphic Design designed it. Allegro Graphics, Inc. typeset the manuscript and Geyer Printing handled the printing and binding.